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INTRA-SERIAL EFFECTS WITH LIFTED WEIGHTS

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An incomplete community of subject-matter of a number of studies concerned with retentive changes in visual material and of others disclosing partially anomalous errors in various forms of the psychophysical judgment has led to the present experiment. Certain errors disclosed by psychophysical studies have in general implied that a change in the value of a past standard stimulus (one which has just been presented) occurs always in the single direction of some still earlier stimulus value or 'average' of stimulus values. On the other hand, numerous forms of changes in retained visual material have been reported. One may ask then: (1) whether there is more than one type of shift of standard stimulus value demonstrable in a modified psychophysical method involving the successive comparison of lifted weights; (2) whether, if several shifts or changes are so demonstrable, to what retentive changes do they correspond; and (3) whether, in seeking to account for any such new psychophysical errors, fruitful reference can be made to configurational principles such as the "Law of *Prägnanz*."

More detailed reference to the psychophysical setting of these problems must be made. Leuba,[1] in following up the related studies of Nichols[2] and Bolton,[3] suggests that there is a natural tendency to shift the sensations held in memory towards the middle of a scale of intensities, or that images tend to approximate a resultant value of all previous images of the same kind.

*Accepted for publication March 15, 1930.

[1] J. H. Leuba, A new instrument for Weber's law; with indications of a law of sense memory, this JOURNAL, 5, 1893, 382-384. The uncommon method of classification is used in the estimation of artificial star magnitudes.

[2] H. Nichols, The psychology of time, this JOURNAL, 4, 1891, 60-112, esp. 78.

[3] T. L. Bolton, On the discrimination of groups of rapid clicks, this JOURNAL, 5, 1892, 304.

Hollingworth[4] infers a corresponding tendency for judgment on the basis of his discovery of an "indifference point" or point in the scale of magnitudes at which there is no error of estimation. Such a point is found to be a function of the upper and lower limits of the total range of magnitudes and appears later[5] in the estimation of sizes of squares. The explanation of the indifference point he named the "Law of Central Tendency," and in a still later work[6] he extended it to a law of reproduction, estimation, identification, memory, perception, habit, and adaptation. Its universality is thus stimulating but confusing.[7]

Let us next note the contributions of the visual, retentive studies to the setting of the problems with which the present study deals. Gibson[8] reviews a number of studies which indicate among other things that visually retained items are subject to change in ways other than "fading" or "blurring." It has been suggested (1) that the reproductions of meaningless, perceived figures (or of meaningful pictures) approximate older, more fixed figures,[9] associated things, standard geometrical forms, represented objects,[10] associated objects,[11] familiar objects,[12] types,[13] and other ideational images in the same or even in another sense-department;[14] (2) that the reproductions of retained visual items change according to cues from a verbal analysis of the originally per-

[4]H. L. Hollingworth, The inaccuracy of movement, *Columbia Contributions, etc.*, 17, 1909, no. 3 (also *Arch. Psychol.*, 1909, no. 13), 31, 35-39, 85.

[5]Hollingworth, The central tendency of judgment, *J. Philos.*, 7, 1910, 461-466, 468.

[6]Hollingworth, *Psychology: Its Facts and Principles*, 1928, 274.

[7]Related to the problems raised by the consideration of the psychophysical errors just mentioned is the discovery of S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 126-150, that with lifted weights a judgment of heavier tended to be followed by a judgment of lighter, and *vice versa*. Not having sufficient evidence, he judiciously made no attempt to decide whether the cause lay in "some contrast effect existing between the pairs of stimuli" or in "a type of expectation tending toward a reversal of judgment."

[8]J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39.

[9]J. Philippe, Sur les transformations de nos images mentales, *Rev. Philos.*, 43, 1897, 486-492.

[10]F. Kuhlmann, On the analysis of the memory consciousness; a study in the mental imagery and memory of meaningless visual forms, *Psychol. Rev.*, 13, 1906, 316-348; also, On the analysis of the memory consciousness for pictures of familiar objects, this JOURNAL, 18, 1907, 389-420, esp. 411.

[11]P. Meyer, Über die Reproduktion einprägter Figuren und ihrer räumliche Stellung bei Kindern und Erwachsenen, *Zsch. f. Psychol.*, 64, 1913, 43 f.; F. Smith, An experimental investigation of perception, *Brit. J. Psychol.*, 6, 1914, 337 ff.; A. R. Granit, A study on the perception of form, *Brit. J. Psychol.*, 12, 1921, 234 f.

[12]F. Wulf, Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psychol. Forsch.*, 1, 1922, 333-373; Gibson, *loc. cit.*

[13]E. B. Titchener, *A Text-Book of Psychology*, 1909, 397.

[14]G. E. Müller, Zur Analyse der Gedächtnisthätigkeit und des Vorstellungsverlaufes: Abschnitt 12, Über die undeutlichen Vorstellungsbilder, *Zsch. f. Psychol.*, Ergbd. 8, 1913. Herein is contained (508 ff.) his "Principle of Convergence" of ideational images.

ceived figure;[15] (3) that they tend to have gaps in themselves completed;[16] (4) that they tend to fall apart;[17] (5) that they tend to become exaggerated with respect to some outstanding aspect of the original;[18] (6) that they change in the two typical directions of "sharpening" and "leveling," both of which are subsumable under the Law of *Prägnanz*;[19] and (7) that they change according to the Law of *Prägnanz*.[20]

It is plain that these studies have had as their main task the isolation of the factors responsible for the changes enumerated. Considering the present prominence of the *Gestalttheorie* it is interesting to note that several of the studies have centered around it and especially around the variously stated Law of *Prägnanz* as an explanatory principle. Köhler's phenomenological form of the law states that "under various conditions characterized by the ineffectiveness of stimulus factors phenomenal, spatial *Gestalten* tend to pass over into simple and precise structures and thus to complete that which was previously irregular or incomplete."[21] Wulf and Werner have offered evidence to the effect that changes occur in the two general directions of "leveling" and "sharpening," which implies that the reproduced figures differ from the originals because of forces operating within the structures themselves, *i.e.* according to a configurational law. Gibson brings forth evidence to the effect that in most cases the change is in the direction of a previous perception,[22] and that the simplicity of Wulf's figures led him to such conclusions about "leveling" and "sharpening." Although Gibson finds in some cases that gaps or breaks in figure contour are either partially or wholly closed up, he takes issue with Wulf with regard to the latter's "structurally conditioned change," saying that there is evidence primarily to the effect that figures change according to (in the direction of, or in the sense of an "approximation to") previous perceptions. With reproduced visual figures his explanation seems most satisfactory. That

[15]Kuhlmann, *loc. cit.*, Gibson, *loc. cit.*

[16]Gibson, *loc. cit.*; cf. Koffka's concept of "closure" in *The Growth of the Mind*, 1925, 250. H. Werner, Über das Problem der motorischen Gestaltung, *Zsch. f. Psychol.*, 94, 1924, 265-272.

[17]Gibson, *loc. cit.*; cf. Müller's "indistinctness of image," *loc. cit.*

[18]Wulf, *loc. cit.*

[19]Wulf, *loc. cit.*; Werner, *loc. cit.*

[20]W. Köhler, Die physische Gestalten in Ruhe und im stationären Zustand, 1920, 263, esp. 250. M. Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.*, 4, 1923, 301-350, esp. 317, 318, 324. Wulf, *loc. cit.*, Werner, *loc. cit.*

[21]Köhler, *op. cit.*, 259 f.

[22] Gibson, *op. cit.*, 33.

leaves the evidence not only from the psychophysical, but also from the visual memory studies, most univocally indicating that retained items either fade or change mainly in the general fashion of approximating some previous perception. The need of a configurational law to account for the phenomena thus seems very doubtful.

It is well for us to ask again: Is there any other form of change of retained items other than one of approximation to previous perceptions; and is it either possible or necessary to relate such a change to the Law of *Prägnanz* or to call it configurational in any sense? The present study attempts to find an answer to these questions.

The experimental procedure is halved. Since the first part deals with changes which apparently occur in the difference-value between the after-effect of the second weight of a first pair of weights and the after-effect of the first weight of a succeeding pair, that part of the study will be called the *Inter-Pair Study*—signifying that the interest is in the intensity interval between the temporally adjacent members of two pairs of weights. Since the second part deals with changes which apparently occur in the difference-value between the after-effect of the first weight of a group of three weights (within which there is a comparison of the second weight to the first, and then a comparison of the third to the second) and the after-effect of the second weight of that group, that part of the study will be called the *Intra-Pair Study*—signifying that the interest is in the intensity interval between two temporally adjacent weights one of which is compared to the other.

The Inter-Pair Study

Apparatus and method. Five subjects were used: Dr. F. A. Pattie (*Pa*), Mr. K. W. Oberlin (*Ob*), Mr. M. E. Carver (*Ca*), Mr. A. J. Harris (*Ha*), and Mr. H. L. Kim (*Ki*).

Weights consisting of smooth, round, card-board boxes 2½ in. in diameter and 1 in. high, and adjusted to the proper values by fillings of lead shot and paraffine, were used as stimuli. These were presented to the *Ss* by use of a turn-table allowing them to be brought to the same position under the right hand. Thus the space error was avoided or at least rendered negligible (according to the slight errors with replacement of weights for each trial). The turn-table was covered with ⅛ in. thickness of felt which was held down by screws and adhesive tape; the felt was marked with white paint so as to leave unpainted circles upon which the weights were placed. The turn-table was mounted on an oiled bearing which rendered it noiseless and smooth in opera-

tion. It was stopped in the proper positions by means of small, rectangular, wooden pegs mounted on its sides; the pegs were the same thickness as a stationary block of wood mounted on the apparatus table so that they just cleared it as they passed over, enabling the experimenter to grasp the peg and stationary block at the same time. This method of stopping was faster than, and practically as accurate as, a sighting method.

The variation of the lifting times was kept within reasonable limits by making use of a telegraph sounder actuated by a Cox disk-relay,[23] which in turn was actuated by one-second break impulses delivered by a Telechron electric timer of the synchronous motor type. The variation of the alternating current supply was within a few hundredths of a cycle per twelve hours; the Cox disk-relay latent period varied within negligible limits; the variation of the *Ss*' lifting was the greatest of all, but a constant watch was kept by *E* so that he could detect and correct obvious lagging or speeding. It can be safely said that variation was negligible. The Cox disk-relay was so set that in the case of the telegraph sounder there was a down-click followed by a one-second interval at the end of which the up-click occurred; then, after a two-second interval, the next down-click, a one-second interval, and an up-click again.

The sounder was kept going constantly during each double-series which involved 20 trials. A hundred trials, or 5 double-series, constituted an experimental hour. Fifty single-series (*i.e.* 25 double series, 500 trials, or 2000 single stimulus presentations), constituted each half or section of the inter-pair study, thus providing 500 cases for the derivation of each limen. (Only 400 cases were used for each limen.)

Three stimulus series were used: a *heavy* series (H), having a standard stimulus of 116 grms., and two comparison stimuli of 108 and 124 grms.; a *light* series (L), having a standard stimulus of 84 grms., and two comparison stimuli of 76 and 92 grms.; and a *medium* series (M), having a standard stimulus of 100 grms., and five comparison stimuli of 92, 96, 100, 104, and 108 grms. respectively.

Within *one half* of the inter-pair study each trial consisted of the presentation of four weights in the following order: H-standard—H-comparison : : M-standard—M-comparison. Within *the other half* of the inter-pair study the trials were made up as follows: L-standard—L-comparison : : M-standard —M-comparison.

The instruction for each half of the inter-pair study was the same:

"At the signal, 'Ready,' you will be presented with two pairs of weights which you are to lift and set down with a simple wrist motion. Each weight is to be lifted immediately after the down-click of the telegraph sounder and set down just before the up-click.

"You are to judge only within pairs, that is, you are to judge the second weight of each pair only in relation to the first weight of the same pair.

[23] A silver-surfaced disk about 3 in. in diameter rotating with a shaft periodically jerked around 1/48 of a complete revolution by electro-magnets operating a ratchet wheel on the same shaft. A thin celluloid disk, having holes punched at practially any desired interval, was clamped over the disk, thus making and breaking the contact with two silver pins resting against the disk and giving the kind of impulses necessary for the actuation of the sounder. The disk-relay was developed by the late Anson G. Cox of the shop of the Harvard Psychological Laboratory.

"Remember the judgment made upon the first pair until the judgment upon the second pair has been made, just as you would if you were to make the usual verbal report.

"Then, after both comparisons have been made, report differences (*i.e.* whether the second weight of each pair was heavier or lighter than the first weight of that pair), by means of the four keys as indicated on the chart before you. If one of the comparisons yields equal, press only a key for the other comparison.

"If both yield 'equal,' report so verbally and press no keys. If you are doubtful of either comparison, report so verbally and press no keys."

A set of four choice-reaction keys accompanied by the following clear legend was used by the *S*s for reporting differences as instructed: 1. First judgment, Lighter; 2. First judgment, Heavier; 3. Second judgment, Lighter; 4. Second judgment, Heavier. Each key actuated a separate Ford ignition-coil (an inductorium having a high tension secondary winding), whose ungrounded secondary terminal converged spatially with the ungrounded secondary terminals of the other three coils to four pins, set near the four corners of a square of fiber, which could be moved to the desired square on the record sheet after the fashion of a slide-rule slider. Underneath the record sheet (directly underneath the course of the movable fiber square), and running its entire height, a thick, brass strip was set into the base of the record apparatus. This strip was connected to the grounded secondary terminals of all four ignition-coils, so that, when any one or two of them were actuated by the report keys, a high voltage electric spark would occur between the proper pin (or pins) and the brass plate, thus burning a discernible hole in prearranged and meaningful squares on the graph record-sheet. (The percentages of judgments were obtained by counting the number of holes by means of a paper key which could be laid over the record sheet.) This method of recording enabled *E* to arrange for the next trial while *S* recorded his own report. While *S* was lifting the weights in a trial *E* would consult the presentation order and shift the fiber square to the proper position on the record-sheet. Although this procedure at first complicated *S*'s task, a number of preliminary trials brought about its effective automatization. Even when the method of report was not automatic there was plenty of time between trials (9 sec.) for *S* to refer to the legend and to press the proper keys. When the experiment got under way the references to the legend were very few. Since there was plenty of time to report, the procedure did not acquire the characteristics of a choice reaction.

A screen placed between the *S*'s eyes and the apparatus prevented him from seeing the weights, the turn-table, the record sheet, or *E*.

Chance orders of presentation of the stimuli, previously determined and recorded, were used at all times.

It is plain that, within one half of the inter-pair study, a pair of weights from Series M was preceded in each trial by a pair from Series H, and that within the other half a pair from Series M was preceded by a pair from Series L. In the case of the preceding pair in the trials in both halves of the study, there were both judgments of 'heavier' and 'lighter,' (and sometimes 'equal'); in other words, in this study the M-standard was preceded an equal number of times by: (1) a heavier weight judged preponderantly 'heavier,' (2) a heavier

TABLE I

INTER-PAIR STUDY. PERCENTAGES OF H, E and L JUDGMENTS UPON SERIES M COMPARISON-WEIGHTS, WITH VARIOUS PRECEDING WEIGHTS

Series	Compar. Weights	Judgment	Percentages of Judgments																	
			Pa				*Ca*				*Ha*				*Ob*				*Ki*			
			H		L		H		L		H		L		H		L		H		L	
			124	108	92	76	124	108	92	76	124	108	92	76	124	108	92	76	124	108	92	76
M	108	H	84	84	92	90	68	74	78	72	56	62	90	80	66	72	92	96	96	80	74	74
		E	12	14	8	10	18	14	10	26	26	18	6	12	26	26	4	4	14	14	24	22
		L	4	2	0	0	14	12	12	2	18	20	4	8	8	2	4	0	22	6	2	4
	104	H	66	54	72	62	40	48	66	58	44	50	74	64	58	42	74	74	82	58	58	66
		E	24	38	26	28	22	38	22	18	30	26	16	22	20	32	20	16	20	6	28	28
		L	10	8	2	10	38	14	12	24	26	24	10	14	22	26	6	10	2	12	14	6
	100	H	58	42	56	54	26	38	44	46	42	26	40	52	20	14	46	54	70	56	56	36
		E	28	44	36	40	36	28	30	16	18	32	30	26	36	30	14	28	14	34	36	38
		L	14	14	8	6	38	34	26	38	40	42	30	22	44	56	40	18	16	10	8	26
	96	H	34	20	30	42	6	14	16	26	30	24	34	36	2	8	8	28	40	42	28	28
		E	20	38	50	40	16	30	30	18	22	28	22	20	6	4	36	18	38	30	40	38
		L	46	42	20	18	78	56	54	56	48	48	44	44	92	88	56	54	22	28	32	34
	92	H	10	14	14	20	10	8	8	10	20	20	28	32	2	2	16	16	32	34	28	18
		E	20	24	54	44	6	12	20	16	10	24	22	22	0	0	22	18	40	34	34	24
		L	70	62	32	36	84	70	72	74	70	56	50	46	98	98	62	66	28	42	38	58

TABLE II

INTER-PAIR STUDY. SERIES M LIMENS WITH VARIOUS PRECEDING WEIGHTS: KIND, AMOUNT AND P.C. OF THEIR DIFFERENCES

Judgment Category	S	Weights Preceding Series M: 76 — Limen and P.E.	76 — Liminal Difference: Kind, Am't and P.C.	92 — Limen and P.E.	92 — Liminal Difference: Kind, Am't and P.C.	108 — Limen and P.E.	108 — Liminal Difference: Kind, Am't and P.C.	124 — Limen and P.E.
Heavier	*Pa*	99.68 ±.48	D − .24 .6218	99.44 ±.21	I − 2.56 1.0000	102.00 ±.34	D − 2.96 .9998	99.04 ±.44
	Ca	101.84 ±.46	D − .08 .5403	101.76 ±.31	I − 1.52 .9853	103.28 ±.35	I − 2.00 .9990	105.28 ±.29
	Ha	99.36 ±.55	I − .56 .6954	99.92 ±.50	I − 4.64 1.0000	104.56 ±.41	I − 1.44 .8976	106.00 ±.94
	Ob	99.44 ±.26	I − 1.68 .9996	101.12 ±.22	I − 3.84 1.0000	104.96 ±.30	D − .56 .8458	104.40 ±.22
	Ki	101.68 ±.33	D − 1.04 .8597	100.64 ±.55	D − 3.60 .9996	97.04 ±.42	D − 3.60 .6063	96.88 ±.34
Lighter	*Pa*	87.84 ±.26	I − 1.04 .9487	88.88 ±.34	I − 5.44 1.0000	94.32 ±.26	I − .88 .9501	95.20 ±.25
	Ca	97.82 ±.39	D − 1.66 .9770	96.16 ±.30	I − .40 .7303	96.56 ±.32	I − 3.36 1.0000	99.92 ±.29
	Ha	92.72 ±.43	I − .51 .7145	93.23 ±.43	I − 1.57 .9046	94.80 ±.69	I − 1.92 .9337	96.72 ±.51
	Ob	95.52 ±.25	I − .40 .7791	95.92 ±.26	I − 3.84 1.0000	99.76 ±.24	I − .72 .9293	100.48 ±.23
	Ki	93.12 ±.38	D − 2.72 .9998	90.40 ±.40	D − .08 .6121	90.32 ±.16	D − 4.40 1.0000	85.92 ±.51

weight judged preponderantly 'lighter,' (3) a lighter weight judged preponderantly 'heavier,' and (4) a lighter weight judged preponderantly 'lighter.'

Let us see what respective effects the four situations just named have upon the percentages of judgment within that pair.

Results and their analysis. In Table I there are recorded the percentages of the various judgments of 'heavier,' 'equal,' and 'lighter' upon the 5 comparison weights in Series M, when judged in relation to the standard of 100 grms., and when that standard has been preceded by the 2 comparison weights of Series H (124 and 108 grms.), and of Series L (92 and 76 grms.). The five wide columns (*Pa, Ca, Ha, Ob, Ki*) separate the results of the five *Ss*. The divisions below (H, L, *etc.*) indicate the series which preceded the standard weight of Series M, and the division below that the particular comparison weight which preceded the Series M standard. At the left of the table it is shown by "M" that the percentages within the table are of the judgments within Series M. The next column (108, 104, 100, 96, 92) indicates the comparison weights of that series. The next column (H, E, L) designates the judgment category for each of the comparison stimuli.[24] Table I is too detailed to indicate any clear tendency.

However, from the data in Table I there have been computed the limens and their probable errors by the use of Newhall's curvilinear interpolation and extrapolation formulae,[25] modified as he suggested in an earlier article,[26] for four comparison stimuli with their accompanying judgment percentage values. The results of the computation have been recorded in Table II,[27] along with other data derived therefrom. The liminal values and their P.E. for the 'lighter' and 'heavier' judgment categories are arranged opposite the names of

TABLE III

INTER-PAIR STUDY. POINTS OF SUBJECTIVE EQUALITY WITH VARIOUS PRECEDING WEIGHTS

Prec. Stim.	*Pa*	*Ca*	*Ha*	*Ob*	*Ki*
124	97.12	102.60	101.36	101.44	91.40
108	98.16	99.92	99.68	102.36	93.68
92	94.16	98.96	96.58	98.82	95.52
76	93.76	99.83	96.04	97.48	97.40

[24]Thus one finds, for example, that *Pa* judged 108 grms. 'heavier' than 100 grms. 84% of the time when preceded by the 124-grm. weight of Series H, and 90% of the time when preceded by the 76-grm. weight of Series L. Likewise *Ob* judged 96 grms. 'heavier' than 100 grms. 28% of the time under the latter condition, and 'lighter' than 100 grms. 54% of the time.

[25]S. M. Newhall, An interpolation procedure for calculating thresholds, *Psychol. Rev.*, 35, 1928, 46-66.

[26]Newhall, Linear interpolation *vs.* the constant process, this JOURNAL, 38, 1927, 390-402. The high correlation of the results of the formulæ used in the present study with results obtained by the use of Urban's tables, and the relative simplicity of Newhall's procedure, have led to the adoption of his formulae.

[27]A further condensation of the contents of Table II has been obtained by tabulating the points of subjective equality in Table III. The column to the left (Prec. stim.: 124, 108, 92, 76) indicates the preceding stimulus values; and the five remaining columns contain the corresponding points of subjective equality for the various *Ss*.

the *S*s in four vertical columns under the legends "Limen and P.E." Above each of these legends is the value of the Series L or H comparison weight which preceded the Series M comparisons from which the limens are derived.

Since the values of the limens for the same *S* differ with different preceding weights, the liminal value as an empirical function of the preceding weight is most significant. In order to detect any uniformity in such a function the liminal difference at each of the three steps between the values of the four preceding weights has been adopted as an indicator. Hence one finds between the four columns of limens three columns, within each square of which are three facts about the difference between the limens in the squares to the left and right of it: the "Kind" of difference, *i.e.* whether passing from left to right in the table it is an increase (I—), or a decrease (D—); the amount (Am't) of the difference; and the probable correctness (P.C.) of the difference.[28] Thus the amount of the difference was an index of its significance. When the amount was small its probable correctness offered a basis—although a highly questionable one—for appraising and comparing the differences. Although the amount and P.C. of a difference are indispensable, that which is most interesting from the standpoint of the present study is whether the liminal difference is an increase or decrease as one passes from the lighter preceding weights to the heavier ones, *i.e.* from left to right in Table II.

Let us see what happens, with various *S*s, to the liminal values at the step from the 76 to the 92 grm. preceding weight.[29] In the cases of the 'heavier' limens we find one case of liminal increase of high probable correctness (*Ob*, P.C. = 0.9996), one case of increase of hardly acceptable P.C. (*Ha*, 0.6954), and two cases of decrease of still less acceptable P.C. (*Pa*, 0.6218, and *Ca*, 0.5403). In the cases of the 'lighter' limens we find one case of increase of high P.C. (*Pa*, 0.9487), one case of decrease of slightly higher P.C. (*Ca*, 0.9770), and two cases of increase of lower but acceptable P.C. (*Ob*, 0.7791, and *Ha*, 0.7145). Thus it can be said that there are two clear cases, two less clear but

[28]Since the liminal differences found were in many cases very small it was felt that some measure of their statistical validity was necessary in order that they could be seriously considered. E. G. Boring, The number of observations upon which a limen may be based, this JOURNAL, 28, 1916, 315-319 (esp. 317), has given directions for obtaining such a measure which he named the "probable correctness of a difference." Applied to the problem of liminal differences, the measure indicates the probability that one limen is greater or less than another, *i.e.* that the two differ. Among the values required for the calculation of the probable correctness of a liminal difference is the P.E. of each limen being considered, and at this point the mathematical procedure of the present study is open to some question. Boring uses the following formula for his calculation: $P.E._L = (0.4769/h)/\sqrt{n}$. Newhall, however, uses formulæ for the P.E. of the limen which are adapted from Culler's more recent formula for the P.E. of the limen derived with the use of Urban's tables. Culler's formula is: $P.E._{CPL} = 0.84535/(h\sqrt{2n})$. In the face of theoretical controversies over the probable error of the limen, and in the face of a need for some mathematical procedure that would place the results of the present study within the realm of a validity which is at least tentatively accepted as such, the writer has ventured to use the new formulae in obtaining an older and better established measure.

[29]A glance at the table shows that the course of the *Ki* values differ markedly as against all the others. Hence it is felt that the results from this *S* deserve a separate discussion later.

obviously acceptable cases, and one not quite acceptable case of increase (a total of four and possibly five cases), as opposed to one clear case of decrease and two cases of decrease of the lowest acceptability of the group. Hence, if only the acceptable cases are considered, the evidence is four to one in favor of increase; and, even if the unacceptable cases were also considered, the evidence is still five to three in favor of increase.

In the passage from the 92 to the 108 grm. preceding weight still more striking differences are to be found. In the cases of the 'heavier' limens there are four acceptable cases of increase, three of which have P.C. of 1.0000. The differences are: *Pa*, 2.56; *Ha*, 4.64; and *Ob*, 3.84 grms. The remaining case (*Ca*) has a P.C. of 0.9853. In the cases of the 'lighter' limens there are four acceptable cases of increase, two of which have P.C. of 1.0000. The differences are: *Ob*, 3.84 and *Pa*, 5.44 grms. The other cases have P.C. of 0.7303 (*Ca*) and 0.9046 (*Ha*). Thus the evidence in eight cases is clearly and completely in favor of increase at this step.

In the passage from 108 to 124 grms. the evidence for increase is slightly less clear. In the cases of the 'heavier' limens there are two acceptable cases of increase with P.C. of 0.8976 (*Ha*) and 0.9990 (*Ca*), and two acceptable cases of decrease with P.C. of 0.8458 (*Ob*) and 0.9998 (*Pa*). In the cases of the 'lighter' limens there are four cases of increase having acceptable P.C.: *Ob*, 0.9293; *Ha*, 0.9337; *Pa*, 0.9501; and *Ca*, 1.0000. The difference in the last case equals 3.36 grms. Thus the evidence is six to two in favor of increase at this step.

It is clear that there is in general an increase in liminal value as the preceding weight increases in value. Since, however, the percentages of the various preceding judgments upon these weights also varied markedly, and, since Fernberger was unable to decide whether preceding judgments or preceding stimulus values could explain his intra-serial effects, one must ascertain whether there is any univocal relation between the preceding judgment percentages and the liminal values of the second judgment.

Thus, in order that the situations preceding the judgments within Series M be clearly understood, the percentages of judgments within Series H and L have been tabulated in Table IV. The extreme left column (H, L) indicates the series under consideration; the next right (124, 108, 92, 76) the comparison weights of these series; and the next right (H, E, L, *etc.*) the categories of judgment pertaining to each weight. The five large remaining vertical columns show the results for the various *S*s. These are each divided into five small columns at the top of which there are designated the comparison weights of Series M which occur in the five possible *succeeding* judgments.

A glance at the table indicates few if any consistent changes in the preceding judgments of a particular trial as they might be affected by the succeeding judgments within that trial. Therefore, with the exception of the percentages of 'equal' judgments, these results have been averaged as shown in Table V. For purposes of relation with Table II the arrangement of the resultant items has been altered and new items added (as mentioned at the end of footnote 30). Thus, disregarding for the time being the three vertical columns labeled, "Difference between Percentages: Kind and Amount," it can be seen that the four remaining numerical columns contain the average percentages of judgment

TABLE IV

INTER-PAIR STUDY. PERCENTAGES OF H, E AND L JUDGMENTS UPON SERIES H AND SERIES L COMPARISON-WEIGHTS

Series	Compar. Weights	Judgment	*Pa*					*Ca*					*Ha*					*Ob*					*Ki*				
			108	104	100	96	92	108	104	100	96	92	108	104	100	96	92	108	104	100	96	92	108	104	100	96	92
H	124	H	86	90	96	90	86	72	82	86	82	52	72	76	56	64	70	96	92	84	84	88	84	88	86	84	86
		E	12	6	4	8	14	24	16	10	14	48	20	16	28	22	14	2	8	12	8	8	4	6	12	14	14
		L	2	4	0	2	0	4	2	4	4	0	8	8	16	14	16	2	0	4	8	4	12	6	2	2	0
	108	H	30	34	38	36	38	24	28	16	22	26	18	26	32	22	32	22	24	18	30	20	52	44	46	42	50
		E	66	52	50	56	36	28	18	18	20	20	32	36	30	34	20	34	20	20	28	36	34	30	26	28	24
		L	14	14	12	8	26	48	54	66	58	54	50	38	38	44	48	44	56	62	42	44	14	26	28	30	26
L	92	H	80	68	76	74	76	86	82	80	82	72	70	64	62	68	62	76	76	76	82	76	98	88	90	98	94
		E	20	20	18	26	20	6	10	14	10	20	20	28	28	14	12	20	18	16	16	18	2	12	10	2	6
		L	0	12	6	0	4	8	8	6	8	8	10	8	10	18	26	4	8	6	2	6	0	0	0	0	0
	76	H	2	0	6	6	12	8	4	10	6	6	10	12	18	10	10	4	6	10	4	4	32	26	38	24	36
		E	34	40	20	36	34	16	18	16	24	20	22	28	18	26	30	20	26	36	22	30	28	48	28	50	24
		L	64	60	74	58	54	76	78	74	70	74	68	60	64	64	60	76	68	54	74	66	40	26	34	26	40

upon the comparison weights of Series L and H for the five *Ss*; the upper section of five lines contains the average percentages of judgments of 'heavier,' and the lower section of five lines the average percentages of judgments of 'lighter.'[30]

Before proceeding it is well for us to notice that the increase in liminal values (Table II) at the step from 92 to 108 grms. is clearly much larger than those at the steps from 76 to 92 and from 108 to 124 grms. The judgment percentages, if operative, would have to account for this condition.

TABLE V

AVERAGES OF THE PERCENTAGES OF TABLE IV, WITH THEIR DIFFERENCES

Percentages of Judgments upon Comparison Weights

Judgment Category	*S*	Series L 76	Difference between Percentages Kind & Am't	92	Difference between Percentages Kind & Am't	Series H 108	Difference between Percentages Kind & Am't	124
Heavier	*Pa*	5	I— 70	75	D— 40	35	I— 55	90
	Ca	7	I— 73	80	D— 57	23	I— 52	75
	Ha	12	I— 43	65	D— 39	26	I— 42	68
	Ob	6	I— 61	77	D— 54	23	I— 66	89
	Ki	33	I— 60	93	D— 46	47	I— 39	86
Lighter	*Pa*	62	D— 48	14	I— 1	15	D— 13	2
	Ca	74	D— 66	8	I— 48	56	D— 53	3
	Ha	63	D— 49	14	I— 30	44	D— 32	12
	Ob	68	D— 63	5	I— 45	50	D— 46	4
	Ki	33	D— 33	0	I— 25	25	D— 21	4

There would seem to be two possible effects of the preceding judgment upon the Series M judgments. (1) When the first judgment has been, for example, 'heavier,' there would be a tendency, due most likely to the verbal-motor rather than to the perceptual mechanism, to say 'heavier' for the next judgment. To the *S* such a tendency might sometimes appear to have the subjective characteristics of 'expectation'—or more likely it would be recognized as a verbal motor tendency to repeat the same word when a different judgment seems imperative but doubtful. (2) On the other hand, something

[30]From Table V it is to be seen in the case of *Pa*, for example, that the percentage of 'heavier' judgments upon the objectively heavier weight of Series H (124 grms.; the percentage is 90) does not equal that of the 'heavier' judgments upon the objectively heavier weight of Series L (92 grms.; the percentage is 75). Such an inequality is seen to exist also with the objectively lighter weights of Series H and L, with all five *Ss*, and with the judgments of 'lighter.' For the purposes of the present study the conditions preceding the comparisons within Series M should vary only in the Series H and L comparison weight values, and not also in the percentage of instances they were judged 'lighter' and 'heavier' than their respective preceding standards. Were the percentage constant, a more facile detection of the effects of the weight values themselves upon the Series M comparison values would seem possible. However, the differences between the adjacent percentages (*e.g.*, for a particular *S*) are so much larger than the above-mentioned, unwonted differences that the effects of the latter are small enough to be neglected for the purposes of the present study.

having the subjective appearance of 'expectation' and tending, as Fernberger has suggested, toward a reversal of judgment may be effective in the judgment percentages obtained in this experiment.

When the 'heavier' limens increase in the step from 76 to 92 grms., as they are shown to do in Table II, it means that *fewer* 'heavier' judgments have been given on the same set of Series M comparison weights. Looking at the corresponding preceding judgment percentage-differences in Table V, one finds that the number of preceding 'heaviers' in all cases *increases* considerably in the step from 76 to 92 grms. This looks like a tendency to reverse the judgment. The 'lighter' limens increase too at this step, a fact which means that *more* 'lighter' judgments have been given on the same set of Series M comparison weights. When one finds the percentages of preceding 'lighter' judgments decreasing markedly in all cases (Table V), the evidence appears to be largely in favor of reversal of judgment. More of the same kind of evidence appears when one notes the same relation between the liminal differences and differences of percentages of preceding judgment at the step from 108 to 124 grms. However, in the step from 92 to 108 grms. there appears a large decrease of succeeding 'heavier' judgments with a decrease of preceding 'heavier' judgments; and, furthermore, an increase of succeeding 'lighter' judgments with an increase of preceding 'lighter' judgments. Such a finding seems to indicate a more marked tendency to repeat the preceding judgment, so that at this point the effect of the preceding judgment anomalously reverses, leading to the rejection of the hypothesis that the preceding judgment is effective.

Since it has been suggested that the absolute judgment may in some way account for the changes in percentage of judgment it is well to consider how it might operate. If, when the Series L weights preceded the Series M weights, there was a tendency to judge the Series M standard as absolutely 'heavy,' a greater number of 'lighter' judgments and a smaller number of 'heavier' judgments upon the Series M comparison weights should result; and there would be a smaller number of 'lighters' and a larger number of 'heaviers' when the heavier weights of Series H have preceded the Series M standard, tending supposedly to cause it to be judged absolutely 'light.' On the contrary, as a glance at Table II will show, the number of 'heaviers' decreases and the number of 'lighters' increases with the step from the Series L to the Series H preceding weights. Since, from what is known of the 'absolute' judgment, one would expect the tendency suggested at the first of this paragraph to exist, a clear explanation of the results of the present study must surely lie elsewhere.[21]

In proceeding it is well to bear in mind that the situation preceding the second judgment of each trial has four characteristic divisions which have been mentioned previously.[22] Now a suspicion arises, both from the work upon the 'central tendency' of judgment and changes in memorized material, and

[21] It is well to note here that G. E. Müller's factor of "speed of lifting" would operate like the 'absolute' judgment. (See his Über die psychologischen Grundlagen für die Vergleichung der gehobener Gewichte, *Arch. f. d. ges. Physiol.*, 45, 1889, 37-112. The standard of Series M would tend to come up quickly after the Series H weights, giving it a lighter comparison status than it would otherwise possess; and likewise, following the Series L weights, it would have a heavier comparison status. Since the results do not indicate any such tendency, Müller's factor can be ruled out as an explanation.

from the likely supposition that the comparison status of the Series M comparison weights is quite stable at the actual time of the comparison as opposed to the quite possible changeability of its immediately preceding standard, that *the Series M standard is subject to influences by the Series H and L weights which immediately precede it.* It must be remembered that the objective value of the Series M standard is 100 grms., a value midway between the heavier comparison weight of Series L (92 grms.) and the lighter comparison weight of Series H (108 grms.). Ordinarily, if the Series M limens are a rather clear function of the preceding weights, as they have been found to be, one would expect to find the function a regular one; instead, what looks like a rather sharp change in it appears at the step from 92 to 108 grms. The position of the Series M standard at the center of this step may not only account for the peculiarity of the function noted, but also may promote a clearer understanding of the rest of it.

Now what happens? When the step from 76 to 92 grms. is made, both sets of limens increase somewhat in value, signifying an increase in the point of subjective equality, *i.e.* in the effective comparison value of the Series M standard. The conclusion then seems warranted that the lighter preceding weight has caused the Series M standard to be lower than has the heavier preceding weight; or, in other words, the preceding weight closer to it in value has caused it to change less than one more distant from it on the intensive continuum. The point of departure for this change of the Series M standard is very likely the value which would be indicated by the time-error, but since this has been kept constant throughout it is not necessary to consider it.

With regard to the step from 108 to 124 grms. a still clearer increase in the values of both sets of limens appears, signifying an increase in the point of subjective equality or in the effective comparison value of the Series M standard. That implies a higher effective value for the Series M standard when it has been preceded by a heavier weight. As before, the weight differing more from the original effective value of the Series M standard has exercised a greater noticeable effect upon it.

Turning to the step from 92 to 108 grms., one finds the most marked increase of all liminal values. It is clear here that the direction of change in liminal value is toward the preceding weight, a fact which signifies that, when the preceding weight is heavier than the Series M standard, the effective comparison value of that standard becomes heavier on the intensive continuum, and that, when the preceding weight is lighter than the Series M standard, the effective comparison value of that standard becomes lighter. Were the Series M standard either greater or less than all of the preceding weights, one would expect to find what could graphically be represented as a single smooth function. As it is one finds what are perhaps three functions showing the respective courses of the limens when (1) the standard weight is preceded by weights lighter than it, (2) when it is preceded by heavier weights, and (3) when the critical change is made from lighter to heavier weights.

From the preceding analysis two general principles can be stated. (1) Under the temporal and other conditions of the present study the direction of change in effective comparison value of a

weight is towards the effective value of a preceding weight. (2) The change is a function of the effective difference between the two weights considered.[33]

The Intra-Pair Study

Apparatus and method. The same *S*s, general manner of presentation, and system of recording as used in the preceding study were employed in this one. There are, however, several significant differences between the two studies. (1) Whereas the problem in the former was one of analyzing further the apparent change of lifted weight after-effect values *in the direction of* preceding values, this study attempts to discover whether there are operative any factors resulting in changes of after-effect values *away from* preceding values; *i.e.* is there a 'peripheral' as well as a 'central' tendency? (2) The arrangement of stimuli differs. (3) The task and performance of the *S*s also differ from those of the preceding study.

In this, as in the preceding study, all the stimuli of Series M were used, but, instead of having a comparison within that series preceded by a pair of stimuli judged in the usual manner, such a comparison was preceded by a heavier weight (124 grms.) or by a lighter weight (76 grms.), *with which the standard weight of Series M was first compared.* Thus in each trial, as before, there were two judgments; but the first judgment was upon the first two weights of the three composing a trial, and the second judgment was upon the last two.

The instructions to the *S*s were as follows:

"You will be presented with *three* weights in succession which you are to lift and set down as before.

"You are to judge the second weight in relation to the first, and the third weight in relation to the second.

"Report in terms of 'Heavier,' 'Lighter,' 'Equal,' or 'Doubtful' as before.

"With both judgments try as before to eliminate the 'equals' as much as possible."

The stimulus differences between the preceding weights and the Series M standard of 100 grms. were large enough to yield nearly always a judgment of difference upon the first comparison; only in 7 or 8 cases of the 500 for each *S* was it necessary to repeat the trial at the end of the study, in order that there might always be the proper difference-judgment preceding the comparison of the last two of each trial. A constant, preceding perception of difference was an important factor to maintain.

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[33]Since Fernberger's experimental situation is sufficiently comparable to that of this inter-pair study, one can easily conclude that the "intra-serial effects with the method of constant stimuli" are explicable by reference to the weight and not to the judgment which precedes a particular comparison. His warning against repetition of particular weight sequences in the method of constant stimuli is now just as significant and somewhat more meaningful. It is well to note here that with any method the use of a *range* of comparison stimuli, not having the standard stimulus value near its median value, will result in a displacement of the standard value effective for comparison towards the part of the continuum along which most of the comparison stimuli lie, and hence in a displacement of the resultant liminal value.

Although it may seem an unusual procedure to have a stimulus (100 grms.) function first as a comparison weight and then as a standard, the logical complexity is nevertheless a psychological simplicity. The *S*s remarked that this procedure was easier for them to carry out than was the one involving four weights and two comparisons. It was described as being like an unseparated pair of steps: "heavier-heavier," "lighter-lighter," "heavier-lighter," or "lighter-heavier."

It is seen that we have in this study two typical situations: the Series M standard was preceded by a weight with regard to which it was always judged

TABLE VI

INTRA-PAIR STUDY. PERCENTAGES OF H, E AND L JUDGMENTS UPON SERIES M COMPARISON-WEIGHTS WITH PRECEDING WEIGHTS OF 124 AND 76 GRMS.

Series	Comp. Weights	Judgment	Percentage of Judgments									
			Pa		*Ca*		*Ha*		*Ob*		*Ki*	
			124	76	124	76	124	76	124	76	139	76
M	108	H	96	86	90	78	52	66	78	94	96	88
		E	4	10	0	6	20	16	16	6	0	12
		L	0	4	10	16	28	18	6	0	4	0
	104	H	92	82	80	56	40	52	42	74	88	60
		E	8	10	4	16	4	6	26	22	6	22
		L	0	8	16	28	56	42	32	4	6	18
	100	H	80	62	40	24	34	32	28	44	78	54
		E	10	18	6	12	8	22	36	38	14	26
		L	10	20	54	64	58	46	36	18	8	20
	96	H	80	36	28	16	28	16	6	22	50	42
		E	6	28	14	6	8	18	40	40	12	22
		L	14	46	68	78	64	72	54	38	38	36
	92	H	58	20	12	8	10	10	2	14	36	28
		E	14	20	2	10	2	14	12	34	24	30
		L	28	60	86	82	88	76	86	52	40	42

'lighter,' or by a weight with regard to which it was always judged 'heavier.' Let us look at the percentages of judgments of the Series M comparison weights under these conditions.

Results and their analysis. In Table VI are recorded the percentages of the various judgments upon the Series M comparison weights in relation with the Series M standard weight, when that standard weight has been preceded by the 76 or the 124 grm. weight, with which it has first been compared. Table VI reads as does Table I, and is likewise too detailed to allow of easy analysis. As the data in Table I have been used to compute the limens and P.E. in Table II, so the percentages contained in Table VI have been used to calculate the limens and their P.E. recorded in Table VII. Furthermore, in Table VII, the kind, amount, and probable correctness of the difference between adjacent liminal values have been computed and recorded as in Table II.

It is to be remembered that in this study a preceding lighter weight is always accompanied by a preceding 'heavier' judgment and a preceding heavier weight always by a preceding 'lighter' judgment; however, since, in the inter-pair study, no consistent effect of the preceding judgment upon the succeeding

one was found, we are not yet able to say that any clear influences of such a nature are operative in the intra-pair study. Let us turn our attention to the values of the preceding stimuli and to the nature of the preceding perception of difference, rather than to the nature or percentages of the preceding judgment.

Ignoring the *Ki* values which will be discussed later, we find in the case of four liminal differences (*Pa* 'heavier' and 'lighter,' and *Ca* 'heavier' and

TABLE VII

INTRA-PAIR STUDY. SERIES M LIMENS AND LIMINAL DIFFERENCES WITH PRECEDING WEIGHTS OF 76 AND 124 GMS.

Judgment Category	Subject	Weights Preceding Series M		
		76		124
		Limen and P.E.	Liminal Difference: Kind, Am't and P.C.	Limen and P.E.
Heavier	*Pa*	98.00 ±.29	D −10.80 1.0000	87.20 ±.29
	Ca	103.28 ±.26	D − 3.12 1.0000	100.16 ±.31
	Ha	104.08 ±.36	I − 3.92 1.0000	108.00 ±.09
	Ob	100.48 ±.24	I − 3.84 1.0000	104.32 ±.28
	Ki	99.52 ±.61	D − 4.24 1.0000	95.28 ±.30
Lighter	*Pa*	94.56 ±.29	D − 5.74 1.0000	78.72 ±.65
	Ca	101.36 ±.26	D − 2.40 1.0000	98.96 ±.26
	Ha	100.40 ±.45	I − 2.24 .9657	102.64 ±.70
	Ob	93.12 ±.31	I − 5.36 1.0000	98.48 ±.36
	Ki	90.24 ±.57	I − 1.92 .9875	92.16 ±.28

'lighter') large decreases in the liminal values at the step from the 76 to the 124 grm. preceding weight. This means that there were many more 'heavier' and fewer 'lighter' judgments on the Series M comparison stimuli when the 124 grm. weight preceded the 100 grm. standard than when the 76 grm. weight preceded it. If the explanation lies as before in a change in value of the after-effect of the 100 grm. standard (*i.e.* in its value which is effective for the second comparison), then one may infer that, when that weight is judged 'lighter' than a weight which precedes it, the effective value of that standard weight changes in the direction of the lighter part of the intensity continuum. In general one can say that the effective value of the weight changes in a direction away from that of a weight from which it has been judged as different.

But in the cases of the two other *Ss* under consideration there appears, as in the inter-pair study, an *increase* in the liminal values at the step from 76 to

124 grms. This apparently calls for reconsideration of the general tendency just proposed. However, it is interesting first to compare the amounts of increase found in the inter-pair study with those found in the intra-pair study. Table VIII contains these amounts of increase (with their probable correctnesses in parentheses) for *Ha* and *Ob*, and indicates clearly that there is less increase when a weight is judged different from a preceding one than when no judgment relation exists between the two. This seems to indicate that a factor similar in operation to that found in the cases of the other two *Ss* is also operating here. However, the results are different under what appears to be

TABLE VIII

SERIES M LIMINAL INCREASES AT THE STEP FROM A 76 TO A 124 GM. PRECEDING WEIGHT IN THE INTER- AND INTRA-PAIR STUDIES

Subject	Limen	Inter-Pair	Intra-Pair
Ha	Heavier	6.64 (1.0000)	3.92 (1.0000)
	Lighter	4.00 (1.0000)	2.24 (.9657)
Ob	Heavier	4.96 (1.0000)	3.48 (1.0000)
	Lighter	4.96 (1.0000)	5.36 (1.0000)

the same stimulus situation, and the situation (*i.e.* the percentages of the first judgment) preceding the second judgment is the same for all *Ss*, facts which would seem to imply that the factor, assumed to be operating to such a large degree in *Pa* and *Ca*, is actually also present with *Ha* and *Ob*, but has probably had its effects neutralized by the operation of other factors. For the sake of convenience let us call the factor whose operation we have inferred from the *Pa* and *Ca* results the *Differentiating Factor*, and its effect in so far as it can be detected the *Differentiation Effect*.

In verifying this implication the possibility occurs that some *Ss* were failing to obey the instructions about comparing the third weight only with the second. It is clear that such a procedure would yield results like those obtained with *Ha* and *Ob*; hence all *Ss* were asked the following question: "In making the second comparison, did you compare the third weight with the second, or did you compare it with the first or some combination of the first and second?" The answers were as follows:

Pa: "Absolutely not. The third was always consciously compared with the second."

Ca: "There was no conscious comparison of the third to the first or to any kind of whole consisting of the first and second together. Once in the introspective series there might have been a comparison into which the first entered, but I am not sure of its exact nature; I am pretty sure that such an occurrence was not common to the main experiment."

Ha: "Occasionally a comparison with the first weight followed that with the second; I have no idea whether it affected the results—it was only an occasional phenomenon. However, I was not able to get a clear comparison

with the second, because I could not get a clear image of what the second was like; I suspect that the first affected it somewhat. For example, sometimes when I got a very heavy first weight, and a light second followed by a third about equal to the second, I would know that the third was lighter than the first, but I was unable to compare it with the second."

Ob: "As far as I recall, I judged the third weight only in relation with the second. The value of the first weight dropped out completely; I get the first weight and hold that value in mind until I get the second weight; the first weight is then dropped out; the first judgment is held in mind and the second weight is set up as a standard; then I hold an 'absolute' value of the second as a standard for the third until I get the judgment between the second and the third—all the while holding the first judgment in mind. It seems as if I have a predisposition to hold judgments and everything facilitating them in mind; useless data drop out."

In the cases of *Pa* and *Ca* we find, as suggested, an insistence upon no tendency to compare the third weight with any other then the second. In the case of *Ha* we find a tendency sometimes to compare the third weight with the first after it had been compared with the second, and that the perceived difference between the third and the first tended to prevent a comparison between the third and the second weights when the difference between them was small. It seems probable that the recognized interference of the first weight is in this case a factor tending to reduce the differentiation effect. *Ob*'s report, as contrasted with those of the other three, does not substantiate the hypothesis; he insisted that the first weight dropped completely out of mind when the second judgment was being made. His description indicates that the first weight did not consciously interfere with the second judgment. Moreover, *Ob*'s P.E. are small and the differentiation effects in his case are the least manifest, facts which subtract from the evidence in favor of remote comparison as accounting for small differentiation effects.[34]

There remains sufficient evidence to support three general statements about the results of the intra-pair study. (1) When a weight has been judged as different from a preceding weight a change occurs in its effective comparison value (as indicated by the point of subjective equality) away from the after-effect value of the preceding weight. (2) The effects indicating this change may be diminished, or even neutralized or reversed by such factors as would work in opposition to the general tendency stated in 1. (3) This study indicates that the most likely factor operating in such a manner is the tendency to compare the third weight of the trials with the first weight.

[34]Further experimentation under conditions more suitable for introspection would doubtless clear up this point for us. For example, if the first weight is heavier than the second and this difference is then increased by presenting a series of first weights heavier still, one should expect to find such results as those found with *Ha* greatly increased; and, on the other hand, we should expect such effects as demonstrated in the *Pa* and *Ca* curves also to be accentuated—these being the reverse of those found with *Ha*. In other words, if the ability and the lack of it to compare the third weight only with the second separate the *Ss* into 'types,' then the typical tendencies should be accentuated by the procedure just suggested.

Analysis of Results for *Ki*

A glance at Table II of the inter-pair study will show that the courses of the *Ki* limens differ markedly from the others. As the objective value of the second weight increases the limens are seen to decrease, a tendency in a direction opposite from that of the majority of the others. In Table VII of the intra-pair study one finds in his case that the 'heavier' limen decreases and the 'lighter' limen increases at the step from 76 to 124 grms.[35]

On page 14 of this article, where the probable effect of absolute judgments of the third weight upon the course of the limens has been noted, it was mentioned that such a tendency would reverse the liminal course as the preceding

TABLE IX

Subject	Mean difference in points of subjective equality	Degree of absolute tendency
Ha	+1.77	1
Ob	+1.32	3
Pa	+1.12	4
Ca	+0.92	(2)
Ki	−2.00	5

weight increased in value. A numerical ranking of the *Ss* on the basis of their reports with respect to the amount of absolute tendency appears in Table IX.[36] Opposite are corresponding values of the mean difference in the point of subjective equality.[37] With the exception of *Ca* there is a tendency for the mean

[35]Since there is such a large degree of uniformity and explicable non-uniformity in the cases of the other four *Ss*, an isolation of the results and discussion of *Ki* has seemed desirable. His judgment percentages indicated an extraordinarily large negative time-error. A requested introspection upon the experience of making the comparison indicated that his arm and wrist muscles felt tense after picking up the first weight, especially if it was a heavy one, and that that feeling of strain remained to be added to the one arising from the lifting of the second weight. Thus the second weight was more often felt as 'heavier.' He was aware of the predominance of 'heavier' judgments. The writer has heard of a *S* in another weight experiment whose introspections and results were similar to those of *Ki*, and who was abandoned at an early stage of the experimentation. However, it is easy to see that with such people there appears a fine opportunity for an objective check upon the presence of pronounced muscle-tonus after work, and upon its relation to the negative time-error of the successive-comparison of lifted weights.

[36]Several sources of error in such usage of the introspective reports are as follows. (1) They were taken several weeks after the main inter-pair experimentation. (2) During the main experimentation the *Ss* were not set to look for absolute tendencies. (3) The estimation and ranking of the introspective reports is open to large variation.

[37]Such a value indicates on the average how much of an increase or decrease occurred in the value of the point of subjective equality (and therefore in the liminal value), when the preceding weight was increased by four grams. The data for its calculation are taken from Table III. For example, in the *Pa* column each value is subtracted from the adjacent value or values. If a particular remainder represents a decrease in the step from a lighter preceding weight to a heavier one it is given a negative sign: *e.g.* 98.16 − 97.12 = (−) 1.04. And if a remainder represents an increase it is accordingly given a positive sign: *e.g.* 98.16 − 94.16 = (+)4.00. The remaining difference, 94.16 − 93.76 = (+) 0.40, is algebraically added to the other two. The mean difference in point of subjective equality is then directly obtained by dividing the algebraic sum (3.36) by the number of differences (3), yielding 1.12, as indicated by Table IX.

point of subjective equality to decrease and to become negative (with increase in objective value of the preceding weights) as the apparent tendency to judge the third weight absolutely increases. This result is accordance in with the proposal made on page 14 of this article and is the best explanation for *Ki*'s performance in the inter-pair study that can be given. It is also partially accounts for the individual differences found in the inter-pair study between the other *Ss*.

Examining Table VII of the intra-pair study, we find that the *Ki* 'heavier' limen decreases at the step from 76 to "124" grms., while the 'lighter' limen increases. This is the only case in the intra-pair study where the two limens take different directions, but, since the first heavier weight in the case of *Ki* was actually 139 grms. instead of 124 grms., the experimental situation hardly allows for a comparison of the *Ki* liminal courses with the others.[38]

Comparing the difference between the points of subjective equality at the 76 to 124 grm. step in the inter-pair study (in this case, 3.76 grms.) with that of the intra-pair study (1.14 grms.), we find that the first is much greater. With the other four *Ss* the tendency is in the opposite direction. Since comparison with the other *Ss* is not valid, these results remain anomalous.

Discussion

As pointed out at the beginning of the present study it is clear at least that memory data change in numerous ways with passage of time, and it has been shown further that these changes can be manifest through their reproduction (*e.g.* in the visual memory studies) and through judgment errors arising from their comparison with other data (*e.g.* in the psychophysical studies). The present study indicates differences in judgment percentages that seem best to be explained by the inference that the value of a standard weight, effective for comparison, changes in two general directions, namely, towards and away from the after-effect value of a preceding weight which one may assume was constant within acceptable limits.

It may be argued that definition of the direction of change in terms of one "towards a previous perception"[39] offers difficulties:

(1) If the previous perception is regarded as that of the immediately preceding weight, Gibson's explanation does not hold, since the value of the standard sometimes changes in a direction away from it, as indicated by the intra-pair study.

(2) If the previous perception is one in which the two members have not been particularly related with one another (*e.g.* in the inter-pair study the

[38]When the *E* found that *Ki* tended to judge the second weight in the intra-pair study (100 gms.) heavier than the first weight being used with the other *Ss* (124 grms.), there was a strong temptation to abandon the procedure within him; but curiosity as to his possible future agreement or disagreement with the others led to the use of the 139 grm. first weight.

[39]P. 3, *supra*.

second and third weights of a trial were not as a rule compared with one another), there is a perceptual totality whose members are not set off against one another, nor are they identified. The exact phenomenal or physiological nature of this totality is difficult to describe. However, if one assumes that to begin with the members have in the least been differentiated, the resultant change is surely not towards such a perception. If they had been identified, then Gibson's explanation might hold; but it is known that this is not the case, since, with the possible exception of *Ha*, the second and third weights in the inter-pair study were differentiated whenever there was a tendency to compare them.

(3) If the perception of difference is regarded as the perception which changes, then, to fit Gibson's explanation, it must change in the direction of a previous perception; but Gibson's problem is then to explain how it could result in a final difference between the first and second weights (such as was indicated in the *Ca* and *Pa* liminal differences of the intra-pair study), which is much greater than any actually perceived difference between any two weights previously lifted in the experiment.[40] His stressed principle of "the turning of new perceptual processes into earlier channels of perceptual activity"[41] does not adequately explain the results of the present experiment.

Are the Law of *Prägnanz* and the principles of "leveling" and "sharpening" any more adequate? Gibson's experimental results have rather demolished the latter, leaving the first mentioned for inspection and use.

Köhler's physiological statement of the Law of *Prägnanz*[42] is hardly useful as yet as an explanation because it assumes a kind of physiology of the central nervous system which is incompatible with much else that is known of the action of the cerebral cortex. And although he relates theoretically the physiological statement to the phenomenological, he introduces into the latter phenomenologically descriptive terms such as "simple," "precise," and "regular" which are objectively intangible.[43] They are comprehensible only—and then with difficulty—by a kind of empathic appreciation of the manual gestures of one who attempts to define them by the use of such gestures and numerous examples. They have not yet been satisfactorily objectified.

Similarly Wertheimer introduces terms phenomenologically descriptive of configurations which have or have not obeyed the Law of *Prägnanz*. "Indefinite" (*unbestimmte*) implies that the figure as such is often mistaken for another figure,[44] but sounds more like the objective statement of "blurring" than it does like the more characteristically configurational "sharpening" or "leveling" as defined by Wulf. Since lack of discriminability implies approximation of the judged data and discriminability implies differentiation, and since the results of the present study indicate two opposed changes of data, the term

[40]Of course, gross differences between compared objects are more common to every day experience, and the perceptions of difference of the intra-pair study may be approximating them. The writer proposes an experiment which is like the intra-pair study, except that at one time the first part of the hour would be taken up with comparison of weights differing greatly, and at another time it would be occupied by the comparison of weights of hardly perceptible differences. If the inference from Gibson's proposed explanation is true the former situation should yield far greater evidences of the differentiation effect than the latter.

[41]Gibson, *op. cit.*, 38.

[42]Köhler, *Physische Gestalten*, *op. cit.*, 250.

[43]P. 3, supra.

[44]*Op. cit.*, 317.

indefinite is not an adequate explanatory tool. "Poor" (*schlecht*) implies lack of correspondence to the precise form (*Prägnanzform*),[45] but that leads back to Gibson's explanation: the *Prägnanzform* is most likely a product of previous experiences. "Inner, mutual possession" (*innere Zusammengehören*) and "good" (*gut*) are used without definition and objective implication.[46]

The Law of *Prägnanz* as variously stated, described, and illustrated fails to find application to the results of the present study.

In the inter-pair study the perception incorporating the Series M standard and any weight preceding it (with which it has not been compared) changes with time so that the members become more like one another. Hence, here is a perception which, if objectively described as being uniquely one of indiscriminability, changes further in the direction of rendering more pronounced the defining characteristic. It changes, not toward any previous perception, but "beyond itself." In the intra-pair study, when the perception incorporating the Series M standard and any weight preceding it (from which it has been judged as different) changes so that the members become more different from one another, one has thus a perception which, if objectively described as being uniquely one of discriminability, changes in the direction of rendering more pronounced *its* defining characteristic.

Such are the implied facts which seem more than distantly related to configurational principles that speak of "perceptual changes arising from the nature of the perception itself." Since the two tendencies are opposed, they remind one vaguely of Wulf's "leveling" and "sharpening" and hint that the present study has succeeded where Gibson shows that Wulf failed, that it has demonstrated memory changes fundamentally independent of past experience. The changes may be configurational, but one hesitates at the term when 'configurational' changes have been so poorly defined. They are not 'functional' in Gibson's sense. They are as yet merely two facts about changes in after-effects involved in the successive-comparison judgment.

Conclusions

(1) When a standard weight is preceded by a heavier weight *with which it is not compared,* a comparison weight following that standard weight will in general be judged 'lighter' than it a greater number of times; and, if the preceding weight is lighter than the standard, the comparison weight following the standard will be judged 'heavier' than it a greater number of times.

[45] *Op. cit.*, 318. [46] *Op. cit.*, 324.

(2) Under these conditions, the greater the difference between the standard weight and the one which precedes it, the greater the resultant preponderance of judgments.

(3) When a standard weight is preceded by a heavier weight in relation to which it has been judged 'lighter,' a comparison weight following that standard weight will in general be judged 'heavier' than it a greater number of times; and, if the standard weight has been judged 'heavier' than a lighter weight preceding it, the comparison weight following that standard will be judged 'lighter' than it a greater number of times.

(4) These effects do not appear to be due to the nature of the preceding *judgment* itself.

(5) Thus far, these effects can best be accounted for by assuming that, by the time the following comparison stimulus appears, the neural after-effect of the standard mentioned has changed in value in the direction of the after-effect value of the preceding weight in the case of (1) and away from it in the case of (3)

(6) With some *S*s the effects stated in (3) can be hidden by the operation of other factors whose presence, however, can be detected and considered.

(7) Regarding the second stimulus of one pair of weights and the first of a pair following as members of a perceived totality characterized by indiscriminability of its members leads, in the light of the assumption in (5) which refers to (1), to the conclusion that the whole has changed so as to accentuate its objective, identifying characteristic: it has become an instance of greater indiscriminability.

(8) Regarding a discriminated pair of weights as a perceived totality characterized by discriminability of its members leads—in the light of the assumption in (5) which refers to (3)—to the conclusion that the whole has changed so as to accentuate *its* objective, identifying characteristic: it has become an instance of greater discriminability.

(9) Regarding the various aspects of the successive-comparison as proposed in (7) and (8) or in any other way fails to render the results of the present study explicable in terms of "approximation of retained data to previous perceptions."

(10) Current statements of the Law of *Prägnanz* fail to gain clarity from or to contribute significance to the results of the present study.

THE EFFECT OF A VISUAL STIMULUS UPON THE PERCEPTION OF BODILY MOTION

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The primary purpose of this study was to obtain quantitative and qualitative data on the effect of a visual situation upon the perception of movement of the body.[1] In the course of the investigation incidental data were secured on some conditions of visual movement. Sine-wave rotary oscillation of the body and sine-wave oscillation of a simple visual object were used as stimuli because they presented a minimum of technical difficulties.

Relativity in the perception of bodily movement is a matter of common experience. Nearly everyone has known the hallucination of his own train moving when the train on the next track started, or the illusion of reversed direction when a more rapidly moving train passed the one in which he was riding. Yet the problem of the influence of vision upon the experience of bodily motion has rarely been subjected to experimentation. Many studies of bodily movement and of seen movement are recorded, but almost no attempt has been made to combine the two situations.

Investigations of the perception of bodily motion have been confined for the most part to passive rotation and to passive rectilinear movement in the horizontal plane. Griffith has summarized the earlier work in rotation.[2] In his own studies of the organic and the mental effects of continuous rotation he found that supra-liminal stimulation (one revolution in 2 sec.) in a modified Barany chair produced an experience of dizziness, which could be analyzed into a large number of pressure-like and kinaesthetic processes that became less intense and less complex with repeated rotation.[3] Later, Dodge,[4] with a refined photographic technique, confirmed in the main Griffith's results and suggested that we ought to speak of central as well as peripheral adaptation to

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[1]The author wishes to acknowledge his indebtedness to Professor Raymond Dodge, Dr. Roland C. Travis, and Professor E. G. Boring.

[2]C. R. Griffith, An historical survey of vestibular equilibration, *Univ. Illinois Bull.*, 20, 1922, no. 5.

[3]Griffith, Organic effects of repeated bodily rotation, *J. Exper. Psychol.*, 3, 1920, 15-46; An experimental study of dizziness, *ibid.*, 89-125.

[4]R. Dodge, Habituation to rotation, *ibid.*, 6, 1923, 1-35.

account for all the facts. In another study,[5] Dodge showed that compensatory eye-movements may be adequate or inadequate in amplitude and in angular velocity, and that visual pursuit-movements tend to dominate the reflex compensatory movements, although with more and more difficulty as rotation-rate increases. Fixation of a motionless field (a field moving with *S*) did not completely inhibit vestibular movements but reduced their amplitude. In an investigation of thresholds of rotation, Dodge,[6] using three different situations, obtained for the threshold values between 1° and 2° per sec., the greatest variation occuring in sine-wave oscillation. By means of a rotating buzzer, independent of the platform, he also demonstrated how auditory factors influence the perception of bodily motion. The *Ss* frequently reported hallucinatory rotation in the stationary periods.

Travis and Dodge[7] recently published an account of work done with rotary and rectilinear oscillation. They used the Dodge chair with sine-wave oscillation for rotary movement, and rolling segments with sine-wave oscillation for rectilinear movement. They found that the percentage of correct responses varied directly, and the percentage of negative and zero responses inversely, as the average velocity and the average acceleration. Visual data gave much finer adjustment to the situation. Kinaesthesis was found to be ten times more sensitive than the vestibule in the perception of rectilinear movement, and the average sensitivity of the vestibule for rotary oscillation seventy times greater than for rectilinear oscillation when expressed in terms of the average velocity of the bony labyrinth, and six hundred times greater when expressed in terms of average acceleration. This result led these authors to believe that the receptors for the two situations are distinct.

Investigation of rectilinear movement has not been so extensive as that of rotation. Mach seems to have done the first experimentation.[8] He concluded that sensations of movement in a straight line are less vivid than those of rotation, that one senses acceleration and not uniform movement, that constant acceleration finally ceases to be felt, that there are after-images of rectilinear movement as there are of rotation, and that an acceleration of 12 cm. per sec. is the lower limit of perceptibility in vertical movement. The sensations seemed to him to come from muscle, skin, bones, ligaments and variations in blood flow within the head, *plus* an additional factor which he decided was mediated by a special organ for rectilinear movement within the head. Delage[9] followed Mach with a study of curvilinear movement which he believed to be subjectively equal to straight-line movement. He came to the conclusion that movement in this situation, whether uniform or accelerated, is perceived in velocity, extent, and duration when the duration is short, but that movement of long duration ceases to be perceived if uniform. Rectilinear movements are less

[5]Dodge, Adequacy of reflex compensatory eye-movements, *ibid.*, 6, 1923, 169-181.

[6]Dodge, Thresholds of rotation, *ibid.*, 6, 1923, 107-137.

[7]R. C. Travis and Dodge, Experimental analysis of the sensori-motor consequences of passive oscillation, rotary or rectilinear, *Psychol. Monog.*, 38, 1928, no. 175.

[8]E. Mach, *Grundlinien der Lehre von den Bewegungsempfindungen*, 1875.

[9]Y. Delage, Appareils pour l'étude du vertige, *L'année psychol.*, 5, 1898 388-390.

finely perceived than movements of rotation, and are mediated by sensations from "a kind of tide of movement of all the parts of our organism which enjoy some mobility," not by a special organ in the head. Bourdon,[10] using a four wheeled wagon which rolled on iron bands and was moved by a system of pulleys and weights, continued this work in rectilinear movement. Greatest sensitivity occurred when the *S* was seated with his back supported. With the body free the values were about the same as with the body secured so as to diminish cutaneous cues from sliding. Bourdon concluded that the cues were primarily not tactile but that they came from inner organs.

The effect of visual factors upon the perception of bodily motion, rectilinear or rotary, has rarely been studied. Warren,[11] in an investigation of conflict between visual and bodily data in rotation, found that vision tended to dominate. The *S* lay on his back on a rotation board with his head raised and his eyes screened so as to permit perception of only a small area of the wall at his feet. Sometimes a mirror was placed so as to reflect the opposite wall and to introduce contrary visual cues. With one to six candles illumination vision strengthened the bodily cues when it accorded with them, and inhibited the bodily cues when it conflicted. With no mirror the slightest movement was perceptible. With the mirror the least perceptible movement occurred between 1° and 2° per sec. More rapid movements with the mirror were interpreted as lateral movement of progression in the direction in which the head was actually moved. With *S* seated over the axis of rotation and with the mirror in use, movements less than 2° per sec. were judged as purely objective, while movements between 2° and 5° per sec. were judged as subjective but in the reverse direction,—that is to say, vision dominated. Movements greater than 5° per sec. were perceived correctly, with the report that the wall was moving in the same direction as the feet but faster.

Travis and Dodge found in their study of oscillation that reactions were much more accurate with than without vision.[12] There were no negative responses with vision, and judgments of extent were more nearly correct. Responses were most adequate when the subject saw the recording lever as well as the floor.

Travis has recently published a study conducted in part at the same time and on the same apparatus as the present investigation.[13] He found that visual cues reënforce the vestibular reflex when ocular pursuit is opposed to the reflex, and that they inhibit the vestibular data when pursuit is in harmony with the reflex. Eye-movement records indicate that pursuit-latency and corrective movements decrease in the harmonious situation, and increase in the opposed situation.

Technique and Methods

Apparatus. The apparatus used in this experiment consisted in the first place of the rotation platform used and described by Dodge in his various rotation studies.

[10]B. Bourdon, La perception des mouvements rectilignes de tout le corps, *ibid.*, 20, 1914, 1-16.

[11]H. C. Warren, Sensations of rotation, *Psychol. Rev.*, 2, 1895, 273-276.

[12]Travis and Dodge, *op. cit.*

[13]Travis, Reciprocal inhibition and reinforcements in the visual and vestibular systems, *J. Exper. Psychol.*, 12, 1929, 415-430.

The platform was driven by a noiseless Pathé spring motor. From the cone pulley of the motor a belt ran to a reducing cone pulley which was connected in turn to a wooden crank pulley 15 cm. in diam. This crank pulley was connected with the platform by a driving rod arranged on an eccentric principle such that, by a sliding sleeve mechanism on the pulley, the stroke of the rod could be varied between 0 and 12 cm. The movement thus obtained was of a sine-wave character, which permitted starting and stopping of each half oscillation without jar. The speed of oscillation was varied by means of the cone pulleys, thus making it unnecessary to alter the speed of the motor.

The apparatus for presenting the visual stimuli consisted of a square hollow tube 15 cm. in diam. and 187 cm. long. It was made out of sheet metal and painted black. Within and one-third the distance from each end were two partitions of black cardboard with holes in the center 6 by 8 cm. to eliminate reflections from the sides of the tube. The tube was suspended in a horizontal position on a level with *S*'s eyes by means of guy wires running from the top of the frame of the rotation platform, and it was pivoted on the axis of rotation directly over *S*'s head. The distal end of the tube was closed except for an opening 8 by 10 cm. over which could be placed the stimulus cards. The stimulus consisted of a hexagonal figure, 2¼ cm. diam., made by punching holes of 1 mm. diam. through a piece of black cardboard at the corners and the center of the hexagon. A symmetrical figure of this sort was adopted because it was thought that it would tend to be less influenced by involuntary eye-movements and would not tend to disappear with continued fixation like a point of light, or change its position when fixation wandered.

In the final experimental series two hexagonal figures were used, identical in size and form, and placed one above the other. One of these figures was kept stationary; the other was oscillated. The stationary figure was supported by a standard which held the card containing it against an opening in the end of the tube.

Fifteen cm. from the end of the tube was placed a medium gray card, which reflected sufficient light through the figure so that it was clearly visible after two minutes of dark adaptation. The room was illuminated by a 75-w. mazda lamp. The tube was driven by the same motor which operated the platform, but through a separate crank pulley, similar to the platform crank pulley and connected to it by chain and sprockets. The driving rod ran from the pulley to a braced framework which dropped from the tube to the level of the two pulleys. Either the chair or the tube could be kept stationary by lifting the proper driving rod from the corresponding pulley and placing it upon a stationary device for that purpose. Extraneous light was eliminated from the tube by means of a hood made out of two thicknesses of heavy black cloth. It was attached to a frame on the platform over the *S*'s head and hung down over his shoulders. The tube was found to be silent and steady in operation, and without vibration.

Frequency in both bodily and visual stimuli was varied by means of the cone pulleys, and amplitude was varied by the sliding eccentric on the crank pulleys.

The auditory factor was controlled by means of an electric fan on the platform just in back of *S*. A piece of paper placed against the blades resulted in

a drone sufficiently loud to smother noises from the apparatus and recording systems and incidental noises from other sources, but it was not loud enough to annoy *S*.

Movements of the platform and of the tube, the responses of *S*, and a seconds time-line were together recorded on a kymograph, revolving at the rate of one revolution in 6 min. A lever made out of thin white pine projected from the platform to the kymograph, which was placed just in front of the platform. A similar lever came out from the framework of the tube and rested upon the drum, just ahead of the platform recorder in order to allow for clearance. Both levers were hinged so that they could be raised in removing the drum.

The method of recording the responses presented a little more difficulty. The nature of the experiment demanded a mechanism which would record independently six different responses: movement of the body right, movement of the body left, no movement of the body, movement of the object right, movement of the object left, and no movement of the object.

The system adopted for recording perceived bodily motion comprised an upright stick just in front of *S*. From it ran strips of wood to the axis of rotation above and below *S*, who held the strips between the thumbs and fingers of his two hands. On each side of this stick were contacts such that a movement of the stick 1 cm. to the right or to the left closed a circuit in which was a two-way marker, so that movement left caused a left deflection of the marker, and movement right caused a right deflection of the marker. A record was thus obtained of the time of occurrence, direction right or left, and duration of bodily movement. A flat spring on either side of the mechanism maintained it in a neutral position except when held right or left by *S*.

The method devised for the reporting of movement of the visual stimulus consisted of two small finger keys arranged on each side of the stick used by *S* to report bodily movement. These keys were operated by the forefingers, the right-hand key for movement of the object to the right and the left-hand key for movement of the object to the left. A two-way electric marker was placed in the circuits. The key was held down as long as movement of the object continued in the same direction. The time of occurrence, the direction right or left, and the duration of movement of the object were thus secured.

Verbal reports after each trial indicated the extent and rate of both bodily and visual perceived movement, and of movement other than right or left.

The *S*s soon became habituated to the response system and reported no difficulty with it. It was superior to purely verbal reports in that it gave objective records which could be quantified with a degree of precision impossible in verbal records, and it eliminated the distraction which vocal responses during movement would have occasioned.

Procedure. Three *S*s were used throughout the investigation, all members of the psychological staff at Yale University and all trained in psychological observation. They are designated by letters, *T*, *N*, and *G*. *T* had been an *S* in the study of rotary and rectilinear oscillation by Travis and Dodge, *N* had been an *S* in one of Dodge's earlier studies in rotation.

The *S* sat astride a bench on the platform with his head against a head rest, so placed that the axis of rotation passed between the right and left vestibular mechanisms.

Reactions to independent bodily and visual movement were first taken to serve as measures of sensitivity and for purposes of comparison. Values near the limen (50% correct responses) were employed. In the trials on bodily movement *S* was instructed to keep his eyes closed and to react to movement right and left by the lever mechanism. When vision alone was used, *S* fixated the object and reacted to right and left movement by depressing the corresponding finger key.

In the next experimental series the bodily and visual situations were combined and *S* was instructed to react to both by means of the dual systems. At first somewhat difficult, with a little practice the task soon became easy and practically automatic.

In the final experimental series *S* reacted to both the bodily and the visual stimuli, but the visual situation was complicated by placing a second hexagonal figure in the field, identical with the first. These figures, one above the other, were separated by 1 cm. when in a vertical plane, and only one of them was oscillated in any given trial. *S* fixated and attended to one of the objects and reported its movement by means of the keys. The behavior of the unfixated object was reported by him verbally at the end of the trial. Sometimes the body was stationary, sometimes it moved with the oscillated object. In all situations vision was binocular.

The trials for the different series were given daily, with a few exceptions during a fixed morning hour, and a daily session usually lasted about forty-five minutes. At least 2 min. were allowed between trials for rest and the fading of after-images, and for changing the apparatus setting. *S* was given a 'ready now' signal and a 'stop' signal. A single trial lasted from 1 to 3 min. and consisted of continuous complete sine-wave oscillations, *i.e.* one period following another without interruption. The presentation was in random order, and each day at least one trial of no movement was given.

After each trial *S* was asked for a verbal report of the experience, and sometimes for a detailed introspective account of some phase of the experience. These reports furnished invaluable data supplementary to the smoked records. Indeed, there were times when the records would have been in some respects incomprehensible had there not been introspections to which to refer.

Results

Treatment. In the reading of the smoked records each complete oscillation was divided into four parts: acceleration right, deceleration right, acceleration left, deceleration left. This treatment gave data on accelerating and decelerating phases, and on movement right and left. The reactions were indicated as positive or negative: positive when movement was perceived correctly, negative when movement was perceived in the wrong direction. Quantitative values were computed on the basis of the total time stimulated.

In the tables, therefore, "88% positive" means that movement was perceived correctly 88% of the time that *S* was presented with the given stimulus; "4% negative" means he perceived movement in the wrong direction 4% of the time. It may be inferred that no movement was perceived the balance of the time. This was usually the case, though sometimes the *S*s reported movement without direction. Directional movement other than right and left was surprisingly rare. Hallucinatory movement was indicated as right or left in the still trials, as with or opposite to the moving factor in the situations in which only the body or the object was oscillated.

(*1*) *Sine-wave oscillation of the body.* Three stimulus values were used, 2° in 4 sec. (*i.e.* from 0° to 2° and return in 4 sec.), 2° in 8 sec., and 2° in 16 sec. The results in percent are given in the upper half of Table I. They indicate a decrease in the amount and the accuracy of perceived movement with decreased average velocity.

Every *S* at every frequency perceived more movement in the decelerating than in the accelerating phase of oscillation, and usually perceived it more accurately. The records indicate a uniform tendency to continue an established response to the end of a half-oscillation. This response almost invariably began in the accelerating phase and continued into the decelerating phase, sometimes to the point of zero velocity, and not infrequently into the beginning of the next half-oscillation. Errors as to direction were more often made and corrected, if corrected at all, in the accelerating phase. Thus the records show greater sensitivity to acceleration, though more positive and less negative movements occur in deceleration.

N and *T* were slightly more sensitive to movement left than to movement right. *G* showed no reliable difference. *N* was left-handed, *T* and *G* were right-handed.

The cues reported were general kinaesthesis, kinaesthesis of torsion from the trunk in rapid movement, organic sensations from the region of the stomach, occasional tactual sensations from contact with the bench, and a pressure-like component from within the head. *N* and *G* reported visual imagery, sometimes of a dark gray extent through which the body moved, sometimes of portions of the room and apparatus. Rapid movement appeared to be of less extent than the slower movement.

(*2*) *Sine-wave oscillation of a visual stimulus.* Four frequencies were used; for *G* and *T*, 2° in 16 sec., 2° in 40 sec., 2° in 65 sec.; for *N*, 2° in 16 sec., 2° in 28 sec., 2° in 40 sec. The positive and negative responses in complete oscillation and in the accelerating and decelerating phases are given in the lower half of Table I. The data show a decrease in the amount and the accuracy of perceived movement with decreased average velocity, as in the case of the body.

The quantitative data and the introspections indicate the influence of the hallucinating visual factor known as autokinesis, which all *Ss* reported in the stationary situations and which tended increasingly to interfere in the movement situations as the average velocity of the stimulus was decreased.

As can be readily seen in Table I, every *S* gave a higher percentage of positive responses and a lower percentage of negative responses in the decelerating phase, as compared with the accelerating phase of complete oscillation. The explanation is the same as that proposed for the bodily data, namely, that

TABLE I

POSITIVE AND NEGATIVE RESPONSES TO INDEPENDENT BODILY AND VISUAL OSCILLATION

			Subjects								
			G			*N*			*T*		
	Stimuli moved 2° in		4 sec.	8 sec.	16 sec.	4 sec.	8 sec.	16 sec.	4 sec.	8 sec.	16 sec.
Body	Complete Oscill'n	+	59	30	10	45	23	21	88	50	30
		−	5	5	4	5	13	20	4	11	10
	Phase Accel.	+	57	26	9	25	22	18	86	43	28
	Phase Accel.	−	3	5	3	8	13	22	3	11	10
	Phase Decel.	+	61	34	10	64	25	25	90	57	32
	Phase Decel.	−	7	5	5	2	13	17	5	11	10
	Stimuli moved 2° in		16 sec.	40 sec.	65 sec.	16 sec.	28 sec.	40 sec.	16 sec.	40 sec.	65 sec.
Visual Stimulus	Complete Oscill'n	+	67	43	29	54	49	37	62	43	42
		−	4	15	12	29	31	27	8	23	26
	Phase Accel.	+	50	36	28	46	41	33	33	39	39
	Phase Accel.	−	7	22	13	36	35	28	15	27	30
	Phase Decel.	+	85	50	30	62	57	41	91	46	46
	Phase Decel.	−	0	8	11	23	27	27	1	19	22

errors tend to occur more frequently and to be corrected in the accelerating phase, and that once the response is established it tends to continue to the end of the half oscillation. (See Fig. 2).

N was more sensitive to movement and to the direction of movement in left periods of oscillation than in right periods, as evidenced by both the correct and the negative responses. *G* and *T* showed no dominance.

The introspective reports show that the extent of movement was always overestimated, and overestimation increased with a decrease of average velocity. At 2° in 16 sec. movement was reported as from 5° to 20°, and at 2° in 40 sec. it was reported as from 40° to 60°. The object accelerated and decelerated quite uniformly at 2° in 16 sec. but moved more irregularly at 2° in 40 sec. These changes may be interpreted as increased interference of visual autokinesis.

(3) Simultaneous sine-wave oscillation of the body and the visual object. The next experimental series was designed to show the experimental consequences of combining the bodily and the visual factors in sine-wave oscillation. The stimuli were the same as those used in the preceding series and have thus already been described.

Eight different situations were used. The results are given in Table II.

(*a*) *Situation 1.* In Situation 1 the body and the object were oscillated together 2° in 8 sec. The perceived body-movement is the same for *N*, slightly less for *G* and *T*, when compared with the same stimulus value without vision (Table I), although there was some improvement in accuracy. An unexpected result is the high percentage of object-movement. The explanation seems to lie in reflex compensatory eye-movements, which occur in spite of pursuit-movements, although reduced in amplitude. Travis has recently submitted comparable data and evidence from eye-movement records pointing in this same direction.[14]

(*b*) *Situation 2.* In Situation 2 the body and the object were oscillated 2° in 8 sec. but in opposite directions. The most interesting fact is the drop in perceived bodily movement as compared to Situation 1, accompanied by a decrease of correct responses and an increase of negative responses.

The introspections are illuminating. The *Ss* had been reporting a tendency to move with the object whenever bodily and visual movement occurred simultaneously, *i.e.* the body and the object seemed to be "tied together." The bodily data and this tendency now come into conflict and the *Ss* reported much confusion, and movement without direction, with a consequent drop in total perceived movements and a loss in correct responses.

(*c*) *Situation 3.* In Situation 3 the body was oscillated 2° in 8 sec., and the object 1.4° in 8 sec. with the body. The movement of the object was in the same direction but of less amplitude, average velocity, and average acceleration than movement of the body. *G* got more body-movement with greater accuracy, and *N* and *T* got less body-movement with less accuracy, than in Situation 1. All *Ss* got less visual movement in this situation than in situation 1, where the body and the object had the same rate and extent. The introspective data reveal periods of confusion like those in Situation 2 but they are less frequent. These differences one may suppose are primarily due to individual differences in eye-movements.

(*d*) *Situation 4.* Situation 4 yielded a higher percentage of perceived movement, bodily and visual, than any other stimulus-form used. The body was oscillated 2° in 8 sec., and the object 2.6° in 8 sec. with the body. Thus the frequencies were the same but the object had a greater amplitude and a greater average velocity than the body. Both body and visual movement were reported as unusually vivid. A comparison of the bodily movement with that in Table I will indicate the degree of reinforcement by the visual stimulus.

(*e*) *Situation 5.* In Situation 5 the body was oscillated 2° in 8 sec., as in Situations 1 to 4, but the object was stationary. In the table the perceived

[14]Travis, *op. cit.*, 1929.

visual movement is represented as 'with' (w) or 'opposite' (o) to the actual body-movement, *i.e.* "w 51" means that 51% of the time visual movement was perceived in the direction of the actual body-movement. Individual differences are again evident, with *N* and *T* showing a loss in accuracy, and *G* and *T* a gain in amount of perceived bodily movement, in comparison with Situation 1. It will be observed that the *Ss* reported object-movement more than half the time, though the object was actually stationary. Of this perceived visual movement *G* got more with the actual body-movement, *T* more opposite to the actual body-movement, and *N* about as much with as opposite to the actual body-movement. These facts point further to differences in eye-movements.

(*f*) *Situation 6.* In Situation 6 the body was oscillated 2° in 16 sec., a subliminal rate, and the object was stationary. The percentages are represented as in Situation 5, 'with' (w) for the visual data meaning in the direction of the actual body movement and 'opposite' (o) meaning in the direction opposite to the actual bodily movement. There was evidently about as much visual movement here as in Situation 5, where the average velocity was twice as great. Part of this movement appears to have been due to the autokinetic factor, which always interfered in some degree where weak stimuli were used. The *Ss* sometimes reported visual movement of 3° to 4°, at other times visual movement of 40° to 60°. The latter movement, which sometimes over-ran several periods of oscillation, they termed a "drift" and reported it to be clearly distinct from the short oscillatory movement. (See Fig. 2). Since this movement was like the movement experienced in the stationary periods, one seems justified in supposing that it was of the same origin (autokinetic). The *Ss* reported in this situation periods of confusion which appear to have been caused by a conflict between the peripheral and the autokinetic factors. Running through the introspections are such terms as "mixed up," "a little of everything," "confusion," "direction of body-movement not clear," "both confused," and "a lack of uniformity in the experience."

The bodily movement experienced in this situation was probably also affected by this autokinetic factor, for whenever visual drifts occurred there was usually bodily movement in the same direction but of less extent and duration than the visual movement. This drift movement of the body was quite distinct from the rather clear-cut oscillatory movement which occurred for near-liminal bodily oscillation. It was of greater extent and duration and the cues were more general and diffuse.

(*g*) *Situation 7.* Situation 7 in Table II presents the percentages of reactions obtained when the body was stationary and the object was oscillated 2° in 40 sec. The visual movement reported for this situation, like some of that reported for Situation 6, was comparable to the visual movement reported for the stationary trials. Amplitudes varied from 40° to 60°. Movement in one direction was usually followed by movement in the reverse direction. The peripheral factor, however, evidently played some rôle in the experience, for *N* got 12% more, *T* 7% more, and *G* 16% less total perceived movement than in the stationary trials (Situation 8); and all subjects got considerably more positive than negative responses.

G and *N* got less total perceived bodily movement in this situation than in the stationary trials. Thus the oscillating visual stimulus seems to exercise at times an inhibitory influence upon the subjective bodily factor.

For the visual stimulus, *N* had a higher percentage of correct responses and a lower percentage of negative responses for left periods, and G a higher percentage of correct responses and a lower percentage of negative responses for right periods. *T*'s responses were about equally divided. For bodily movement, *N* and *T* got as a rule a higher percentage of correct responses for movement left, while *G* showed no consistent dominance. These results are comparable to those obtained for right and left periods of oscillation of the body without vision, and for the object without the body.

(*h*) *Situation 8.* In Situation 8 the body and the visual stimulus were both stationary.

With regard to the acceleration and the deceleration phases of complete oscillation, the same general results were found as when the body and the object were used independently, *i.e.* more positive and less negative responses occurred in the deceleration phase. The relative difference, however, was greater for the body than for the object. In every case but one the percentages of correct responses for the body in the accelerating phase were more than doubled in the decelerating phase. The visual stimulus very evidently increased the tendency to experience more bodily movement in the decelerating phase. This effect was likely due to the bodily data being delayed by prior entry of the visual sensations.

The introspective accounts indicate that the vividness and the extent of the perceived bodily movement in all of the situations were comparable to the vividness and the extent of the perceived visual movement. The bodily cues were chiefly a general kinaesthesis, kinaesthesis from the back and shoulders and sometimes from the eyes, organic sensations of a faint, lively nature from the region of the stomach, tactual sensations from contact with the bench, and sometimes a 'pressury' experience within the head.

Visual extents where reported were almost invariably overestimated. In Situation 1 movement was usually between 8° and 15°, in Situation 2 between 8° and 20°, in Situation 3 between 3° and 10° (except for an infrequent 40° to 60° movement, probably autokinetic), in Situation 4 between 10° and 20°, in Situation 5 between 1° and 10° (infrequently 40° to 60°), and in Situations 6, 7, and 8, between 40° and 60°.

Variability. Fig. 1 represents the daily variation of positive reactions to bodily oscillation at 2° in 8 sec. with and without vision, for subjects *G* and *T*. In the visual situation the object

TABLE II

REACTIONS TO SIMULTANEOUS BODILY AND VISUAL OSCILLATION

(B = body; V (and V.St.) = visual stimulus; w = perceived visual movement with actual body-movement; o = perceived visual movement opposite to body-movement; for Situations, see text.)

Situation		Subjects					
		G		*N*		*T*	
		Body	V.St.	Body	V.St.	Body	V.St.
1	B 2° in 8 sec.	+ 27	+ 69	+ 31	+ 68	+ 45	+ 65
	V 2° in 8 sec.	− 3	− 3	− 6	− 7	− 4	− 3
2	B 2° in 8 sec.	+ 4	+ 70	+ 11	+ 77	+ 36	+ 72
	V 2° in 8 sec.*	− 17	− 8	− 14	− 6	− 7	− 4
3	B 2° in 8 sec.	+ 33	+ 60	+ 20	+ 47	+ 31	+ 35
	V 1.4° in 8 sec.	− 2	− 4	− 10	− 13	− 11	− 9
4	B 2° in 8 sec.	+ 35	+ 71	+ 38	+ 82	+ 47	+ 78
	V 2.6° in 8 sec.	− 3	− 4	− 2	− 5	− 4	− 4
5	B 2° in 8 sec.	+ 37	w 51	+ 20	w 34	+ 41	w 18
	V stationary	− 5	o 17	− 14	o 32	− 13	o 33
6	B 2° in 16 sec.	+ 16	w 22	+ 16	w 31	+ 23	w 24
	V stationary	− 18	o 43	− 16	o 33	− 10	o 31
7	B stationary	w 19	+ 31	w 9	+ 39	w 11	+ 42
	V 2° in 40 sec.	o 8	− 11	o 4	− 22	o 16	− 18
8	B stationary	R 23	R 49	R 10	R 13	R 14	R 24
	V stationary	L 10	L 9	L 22	L 36	L 12	L 29

*opposite

oscillated 2.6° in 8 sec. with the body (Situation 4, Table II). B in the figure indicates the curves for perceived body-movement without vision, B+V the curves for perceived body-movement with vision. Day to day variation is evident in both situations. Only the body-object situation, however, shows learning. Thus

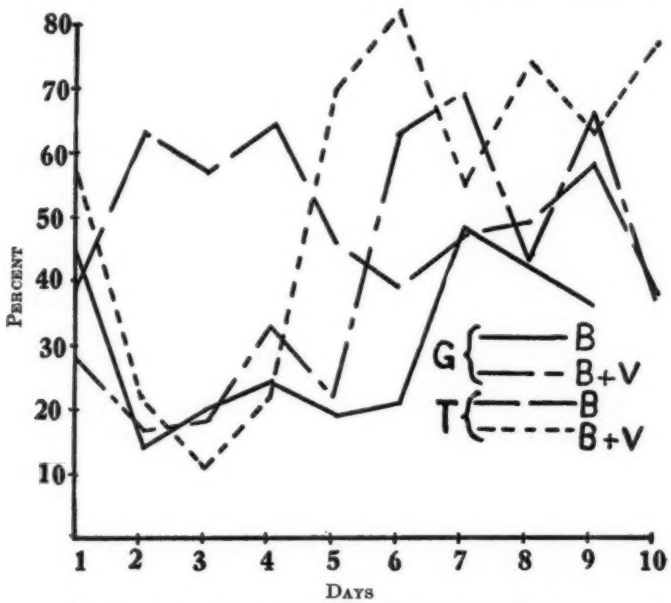

FIG. 1. DAILY VARIATION OF POSITIVE BODILY MOVEMENT
B: Bodily movement without vision. B + V: bodily movement with vision. Subjects *G* and *T*.

we have evidence that vision exercised a practice effect as well as an immediate effect upon perceived bodily movement.

(*4*) *Bodily and visual oscillation with two visual stimuli.* In this experimental series two different velocities were used, 2° in 8 sec., a rate near the bodily threshold for sine-wave oscillation of the body and one used in the preceding series, and 2° in 40 sec., a rate slightly below the threshold for sine-wave oscillation of the simple visual stimulus. The two objects used were identical hexagonal figures, exactly like the single figure used in the other series. The results are given in Table III.

Table III

Reactions to Body and Fixated Visual Stimulus

(F and F.V.St. = fixated stimulus; U = unfixated stimulus; B = body; w = perceived visual or body-movement with actual movement; o = perceived visual or body-movement opposite to actual movement; for Situations see text.)

Situations		Subjects					
		G		*N*		*T*	
		Body	F.V.St.	Body	F.V.St.	Body	F.V.St.
1	B 2° in 8 sec. F 2° in 8 sec. U stationary	+ 48	+ 55	+ 34	+ 83	+ 82	+ 90
		− 31	− 10	− 10	− 2	− 1	− 1
2	B 2° in 8 sec. F stationary U 2° in 8 sec.	+ 17 w	5	+ 27 w	4	+ 32 w	1
		− 23 o	57	− 13 o	77	− 9 o	90
3	B stationary F 2° in 8 sec. U stationary	w 25	+ 69	w 34	+ 86	w 53	+ 92
		o 23	− 4	o 12	− 3	o 0	− 1
4	B stationary F stationary U 2° in 8 sec.	w 19	w 16	w 1	w 2	w 1	w 3
		o 33	o 34	o 46	o 82	o 51	o 81
5	B 2° in 40 sec. F 2° in 40 sec. U stationary	+ 47	+ 45	+ 12	+ 83	+ 31	+ 51
		− 38	− 26	− 5	− 3	− 33	− 24
6	B 2° in 40 sec. F stationary U 2° in 40 sec.	+ 57 w	30	+ 4 w	3	+ 37 w	26
		− 31 o	46	− 28 o	78	− 38 o	50
7	B stationary F 2° in 40 sec. U stationary	w 32	+ 45	w 20	+ 84	w 34	+ 52
		o 48	− 19	o 2	− 2	o 35	− 26
8	B stationary F stationary U 2° in 40 sec.	w 36	w 27	w 4	w 4	w 20	w 18
		o 41	o 48	o 44	o 85	o 31	o 65

(*a*) *Situation 1.* In Situation 1 the body and one of the objects oscillated synchronously 2° in 8 sec. in the same direction, the other object being stationary; and *S* fixated the moving object. A comparison of the perceived bodily movement in this situation with that in Situation 1 of the preceding series (Table II) shows that all *Ss* got more correct movement here than when the single object was used. *G* and *T* nearly doubled their positive movement; *T*, *N*, and *G* increased their negative movement. This increase of negative responses, the introspections indicate, was largely due to diversion of the attention to the unfixated object, for attention sometimes shifted from the fixated to the unfixated object in spite of efforts to prevent it. The direction of bodily movement changed correspondingly when the shifts occurred and the *S* seemed to move with the unfixated object. *T* reported less movement of the unfixated object, rarely more than a degree, and infrequent shifts of attention. His negative responses are correspondingly lower. *G*'s body-movement was also affected by the autokinetic factor.

Both *N* and *T* show an increase in correct responses and a decrease in negative responses for perceived visual movement, while the reverse is true of *G*. Were it not for the introspections it would have been difficult to account for *G*'s results. In this situation he frequently experienced the "drift" movement, characteristic of the stationary trials. During these drifts the body and the two objects seemed to be moving in a great arc of 40° to 60° amplitude, and 20 to 40 sec. duration, while the two objects oscillated opposite to each other independently of the drift. The drift was usually more vivid and rapid when the fixation object oscillated in the direction of the drift. The other *Ss* also reported this phenomenon, but less commonly at this average velocity. At 2° in 40 sec. *T*, like *G*, reported it as very frequent. We evidently have here the operation of the autokinetic factors.

(*b*) *Situation 2.* Situation 2 was the same as Situation 1 except that fixation and attention were upon the stationary object. The results are particularly interesting. The stationary object is seen to move opposite to the unfixated object (in this case also moving) most of the time. Obviously visual movement is relative. It is probable that the unfixated object has reduced the extent of the compensatory and pursuit eye-movements, which in this instance would reënforce one another. The extent of the visual movement was reported as from 4° to 10° for a half-oscillation. Sometimes the unfixated object displayed greater amplitude than the fixated, but not as a rule.

An examination of the perceived bodily movement shows considerable change from that found in Situation 1. The correct responses decrease for all *Ss* while negative responses increase for *N* and *T* and remain relatively high for *G*. All *Ss* reported that it was at times impossible to keep attention upon the fixated object. Attention sometimes shifted to the unfixated object and then bodily movement appeared to be with it. Shifts were induced when the unfixated object moved more extensively and vigorously than the fixated, or when the bodily data indicated the body was moving with the unfixated and opposite to the fixated object. Sometimes bodily movement with confused direction occurred, probably because of conflict between the two tendencies.

A comparison of this situation with Situation 5 in the preceding series, in which the body oscillated 2° in 8 sec. and the object was still (Table II), shows

the consequence of adding an unfixated visual stimulus oscillating with the body. There is a considerable increase in movement of the stationary object opposite to the actual body-movement. Bodily movement was also affected but in no constant direction because of the involuntary shifts of attention to the unfixated object and consequent movement of the body with that object.

At least three eye-movement tendencies were probably at work in these two situations of this series, reflex compensatory movements, pursuit-movements, (conflicting with the reflex movements in Situation 1 and reënforcing them in Situation 2), and involuntary attraction of the line of regard by the unfixated moving object. The consequences in any *S* at any particular time probably depended upon the relative strengths of the three tendencies.

(*c*) *Situation 3.* In Situation 3 the body and the unfixated stimulus were stationary and the fixated stimulus oscillated 2° in 8 sec. The percentages of correct responses for visual movement are slightly higher than in Situation 1 of this series. Except in *N*'s case, there is less perceived bodily movement, though all *S*s get bodily movement practically half of the time, usually in the direction of the actual visual movement. *G*'s higher percentage of bodily movement in the reverse direction is due to the autokinetic factor and to shifts of attention to the unfixated object. *N*'s percentage of reverse bodily movement is also primarily due to attention shifts. *T* got no reverse bodily movement, and reported little movement of the unfixated object and no attention shifts.

(*d*) *Situation 4.* In Situation 4 the stimulation was the same as in Situation 3, but fixation was upon the stationary object. The percentages show that the *S*s do not get as much movement of the stationary object as they did of the moving object in Situation 3, though the differences are slight for *N* and *T*. Movement of the body was perceived about half the time in this situation, most frequently, as the introspections and the percentages show, in the direction of the fixated object and opposite to the unfixated (the actually moving) object. *G*'s bodily movement with the unfixated stimulus was caused evidently by shifts of attention to the unfixated object, which in his case was seen to move more vividly and extensively than the fixated object, and by the autokinetic factor.

The bodily cues most frequently reported in Situations 1 to 4 were general kinaesthesis, kinaesthesis from the eyes and trunk, contact from the bench, faint bright pressures from the region of the stomach and diaphragm, and a 'pressury' experience within the head. Practically the same cues were reported in Situations 3 and 4, where no actual body-movement took place, as in Situations 1 and 2, though general unlocalizable kinaesthesis was more common.

(*e-h*) *Situations 5-8.* Situations 5, 6, 7, and 8 respectively were comparable to Situations 1, 2, 3, and 4 described above except that the rate of movement was much slower. One of the visual objects always oscillated 2° in 40 sec. The results for *G* and *T* were considerably affected by the autokinetic factor. *N* rarely reported "drifts," and his results are probably therefore more typical of chiefly peripheral determinations. The influence of autokinesis on the percentages of bodily and visual movement of *G* and *T* is seen in the much greater leveling between *plus* and *minus*, 'with' and 'opposite' percentages than is present in *N*'s case. The drifts were like those reported by *G* in Situations 1 to 4 of this series, except that the independent oscillation of the objects was at

a much slower rate. The two objects and the body would move in an arc, usually from 40° to 60°, while the objects oscillated 10° or 15° opposite to one another and independently of the drift. The drift was commonly more rapid and vivid when the fixated object moved in the same direction and would usually slow up (sometimes momentarily stop) when the fixated object swung back in the reverse direction. There were times, however, when attention would shift to the unfixated object as the fixated object began to reverse, and the unfixated object would then carry the drift along with undiminished velocity until the fixated one completed its reverse movement and began to return. *G* and *T* knew that such movement was impossible under the experimental conditions, but all voluntary efforts to prevent it were found to be futile.

A comparison of the percentages for visual movement in Situations 5 and 6, which were alike except for fixation and attention, shows that there was as much movement perceived in the stationary (Situation 6) as in the actually moving object (Situation 5), though in the reverse direction. The introspections are confirmatory. Situations 7 and 8 indicate that this is also true when the body is stationary. In either case *S* is unable to tell which object is actually moving, a result which lends further experimental confirmation to the fact of the relativity of visual movement.

With regard to perceived bodily movement, it is obvious that *G* and *T* get considerably more than *N*. This is because of their autokinetic tendency. There is evidently for them little difference between the situations in which the body actually moved and those in which it did not.

N's results show the tendency for the body to move with the fixated object. This tendency is substantiated by his introspections. The body-movement was so far below the limen that the bodily data rarely conflicted with this tendency, as it frequently did in Situation 2. Curiously enough, N got more bodily movement when fixating the stationary object than when fixating the moving object, and more bodily movement when the body was still than when it was actually oscillating.

A comparison of Situations 7 and 8 with Situation 7 of the preceding series (Table II) is interesting in that it seems to indicate the effect of the secondary object. All *S*s have an increase in both bodily and visual movement. Part of the effect however is probably due to practice.

Fig. 2 shows some characteristic smoked records. The reaction line for the object is above and that for the body is below the time line. The two are in alignment with each other in all records, and also with the upper curve (object curve) in records A, D, and E.

A represents *N*'s responses to the fixated stationary object and the body when the body and the unfixated object oscillated together 2° in 8 sec. (Situation 2, Table III). Half-way through the situation the bodily data, aided by a shift of attention to the unfixated object, finally dominate the tendency to move with the fixated object.

B indicates *G*'s reactions when the body and the fixated object were still, and the unfixated object oscillated 2° in 8 sec. (Situation 4, Table III). The objects were reported as oscillating opposite to one another, the fixated 3°, the unfixated 5°. Autokinesis appears in the last half of the situation.

C indicates *G*'s reactions in one of the periods of no movement in the single-object-body series (Situation 8, Table II). The inter-

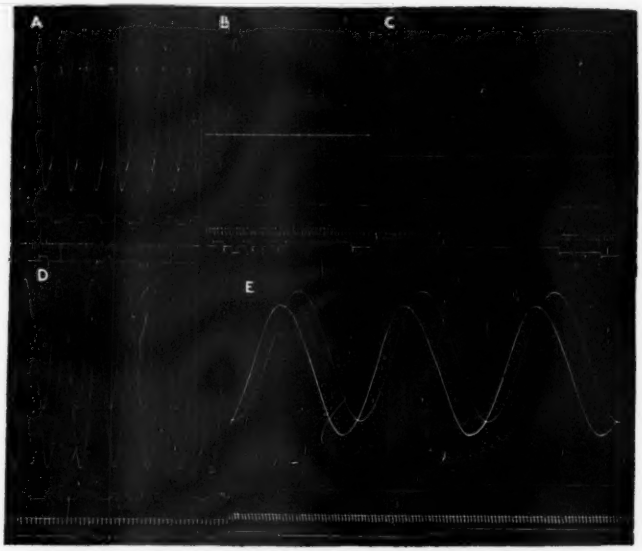

FIG. 2. TYPICAL RECORDS

The upper sine-curve is the record of oscillation of the visual object. The lower sine-curve is the record of oscillation of the body. The time-line (secs.) is next to the bottom record. Above it is the *S*'s reaction to the visual stimulus; below it is the *S*'s reaction to bodily movement. The five situations represented are as follows:

Record	Subject	Movement of body	Movement of fixated stimulus	Movement of unfixated stimulus
A	*N*	2° in 8″	stationary	2° in 8″
B	*G*	stationary	stationary	2° in 8″
C	*G*	stationary	stationary	no second stim.
D	*T*	2° in 8″	2° in 8″	no second stim.
E	*T*	2° in 40°	stationary	2° in 40°

relation of bodily and visual autokinesis is evident. The reverse drift that appears at the end of the record was broken off by the 'stop' signal.

D represents *T*'s responses in the single-object-body series when the body and the object oscillated 2° in 8 sec. in opposite directions (Situation 2, Table II). Conflict between the bodily data and the tendency to move with the object is evident, with the bodily data finally becoming dominant. The periods of no bodily movement at the beginning and towards the end of the situation were reported as "confused."

E indicates *T*'s reactions when the body and the unfixated object oscillated 2° in 40 sec. and the fixated object was stationary (Situation 6, Table III). Bodily and visual autokinesis are evident with the fixated object, which was seen to move opposite to the unfixated, starting each of the two drifts.

Discussion of Results

Experienced movement, bodily or visual, appears to be a meaning that may attach to one or more of several visual and bodily factors.

When the body and the head are perceived as still, perceived movement of an object may be mediated by retinal displacement, kinaesthesis of eye-movements of pursuit, kinaesthesis of fixation interpreted as pursuit, visual imagery of a spatial frame, or a complex of these factors. We have found that the accuracy of perception increases with the velocity of the stimulus, the limen being around 0.13° per sec. average velocity, a value comparable to the values obtained by Aubert[15] and Bourdon.[16] These investigators found the limen to be much lower, 1-2 min. per sec., when objective points of reference with a stationary meaning were used.

When two small objects without a well established stationary meaning make up the field of vision, movement of one produces perceived movement of both in opposite directions, regardless of whether fixation is on the stationary or the moving object. Fixation and attention—attention is the real determiner—tend to make either object more mobile than when unfixated. Thelin[17]

[15]H. Aubert, Die Bewegungsempfindungen, *Arch.f.d.ges.Physiol.*, 39, 1886, 347.
[16]B. Bourdon, *La perception visuelle de l'espace*, 1902, 185.
[17]E. Thelin, Perception of relative visual motion, *J. Exper. Psychol.*, 10, 1927, 321-349.

obtained similar results. The supposition found in textbooks that the fixated object in an ambiguous visual situation is seen as stationary is therefore not correct. In the moon-cloud illusion we have a case in which one object assumes movement of another, fixation increasing mobility of the moon. The disparity in size and intensity probably facilitates the tendency, as some results of Carr and Hardy appear to indicate.[18] A facilitating factor in such cases is probably visual autokinesis. We have seen it tends to interfere considerably at low velocities.

Perception of passive bodily rotation with the eyes closed may be mediated by tactual clues, kinaesthesis from the trunk and limbs, organics from the viscera, kinaesthesis of reflex eye movements, pressure from within the head, a general, faint, lively pressury component, and a visual image of spatial extent. We found, like Travis and Dodge,[19] that the accuracy of perceived bodily movement in rotary oscillation decreases with decreased velocity and that the limen falls at about 0.75° per sec., a somewhat lower value than Dodge[20] found for continuous rotation.

Mach believed the semi-circular canals were the sense-organs of rotation. There is a mass of confirmatory data, though the present belief is, following Griffith,[21] that the awareness of rotation is an awareness of reflex activity induced by canal excitation. On Mach's theory, acceleration produces an experience of rotation in the same direction, deceleration an experience of rotation in the reverse direction, because of the dynamics of the canals. Our data indicate that correct responses invariably begin in the acceleration phase of sine-wave rotary oscillation, thus tending to substantiate this theory. Deceleration in sine-wave rotary oscillation rarely produced, however, a movement in the reverse direction. One turns, therefore, from the periphery to the center.

It appears probable that a persistence of the central process aroused in the acceleration-phase inhibits the antagonistic peripheral data which occurs in the deceleration phase. Comparable phenomena are known, as in prior entry.

The smaller percentages of correct responses in the acceleration phase may have been due to the negative effect of weak stimuli at

[18]H. A. Carr, and M. C. Hardy, Some factors in the perception of relative motion, *Psychol. Rev.*, 28, 1920, 24-37.
[19]R. C. Travis and R. Dodge, (*op. cit.*), *Psychol. Monog.*, 38, 1928, no. 175.
[20]Dodge, (*op. cit.*), *J. Exper. Psychol.*, 6, 1923, 107-137.
[21]C. R. Griffith, (*op. cit.*), *ibid.*, 3, 1920, 89-125.

the beginning of acceleration, such as Newhall and Dodge found in color vision,[22] or an inhibitory effect of the persistence of the just previous excitation. Responses sometimes ran over into the succeeding half-oscillation. The excess of negative responses in the acceleration phase of bodily and visual oscillation was partly due to this reaction lag, but also momentary misinterpretation of directional cues when movement first appeared in a half oscillation. Movement often preceded direction at such times. Dodge for rotation, and Travis and Dodge for rotary oscillation, have also reported non-directional movement.

Negative movement sometimes continued through a half-oscillation in both visual and bodily movement, and the percentages of negative responses increased with decreased velocity. Sometimes a single reaction would spread over several half-oscillations. The most likely explanation of such phenomena is in visual and bodily autokinesis. Our data, quantitative and qualitative, have pointed clearly to the operation of autokinetic factors, increasingly active with decreased velocity. Travis and Dodge suggested a central factor to account for the negative movement they found in rotary oscillation.

When both the body and the visual field are moved, the perception of movement, visual, bodily, or both, becomes quite complex. Many cues may be present, visual and bodily, with consequent interaction, conflict, facilitation and inhibition. Movement-reference may be to the object, to the body, or to both. When *S* is set for both, both tend to move when either moves. Vision usually dominates, however; seen movement affects the frequency and extent, and largely determines the direction, of bodily movement. There is a marked tendency for the body to move with the object fixated and attended to. This may be an expression of what appears to be a common determining tendency, the tendency for the body to follow the line of sight. When the body actually moves in a reverse direction, conflict frequently occurs between this tendency and the peripheral data, producing moments of confusion, unpleasantness, non-directional movement, and decreasing the amount of reported movement.

With such a multiplicity of interacting bodily and visual cues, the variability found among and in *S*s is not puzzling. Mental

[22] S. M. Newhall and Dodge, Colored after-images from unperceived weak chromatic stimulation, *ibid.*, 10, 1927, 1-17.

set, shifts of attention, *Aufgabe* evidently at times strengthened certain factors as over against certain other factors. There were obvious individual differences in the strength of visual and bodily autokinesis. Prediction in the case of any given stimulus was impossible. In one instance no movement might occur, in another negative movement, in a third correct movement. In general prediction was less uncertain for supra-liminal stimuli, perhaps because interference by autokinesis was not so likely.

In any ambiguous movement-situation (*e.g.* two adjacent trains of which one starts) it appears that the movement-reference may take any one of three directions, depending upon the factors which happen to be dominant at the moment. If attention is upon one's body, and expectancy of starting has perhaps partially excited a bodily autokinetic tendency, movement of the adjacent train may be referred to one's own train. If, however, the attention, and perhaps fixation, is upon the next train, and bodily autokinesis has not been prepared by expectancy of immediate bodily movement, the movements of one's own train, if at low acceleration, may be referred to the other train. With a divided set, or a balance between expectancy, on the one hand, and attention, on the other, both may be perceived to move when either moves. Thus we found in the present study that movement of either body or object in the visuo-body situation, where attention and expectancy included both body and object, gave perceived movement of both.

In every-day life the influence of objects of well established stationary meaning plays a dominant role in our movement-perceptions. Thus the train moves rather than the landscape, the lamp in the ship's cabin sways rather than the cabin, unless bodily data make the body a point of reference, and the swing turns over in the 'haunted swing' illusion,[23] not the room.

Conclusion

(1) In rotary oscillation of the body at 2° amplitude, the percentages of correct responses varied directly, and the percentages of negative responses tended to vary inversely, as the average velocity. The limen, defined as that value at which 50% correct responses occur, averaged for the three subjects for the body 0.75° per sec. mean velocity.

[23] R. W. Wood, The 'haunted swing' illusion, *Psychol. Rev.*, 2, 1895, 277 f.

(2) A higher percentage of correct responses occurred for all *Ss* in the decelerating phase of bodily oscillation than in the accelerating phase.

(3) In oscillation of a simple visual stimulus at an amplitude of 2°, the percentage of correct responses varied directly as the average velocity for values around the limen. The limen averaged for the *Ss* 0.13° per sec. mean velocity, or about one-sixth the liminal value for the body.

(4) A higher percentage of positive responses occurred in the decelerating phase of visual oscillation than in the accelerating phase.

(5) When the *S* fixated the simple visual stimulus while the body was oscillating at a near-liminal value, the amount of perceived bodily movement was found to depend upon the relative movement of the visual stimulus. Most movement was perceived when the object oscillated with the body but at a greater amplitude.

(6) A marked tendency existed for the body to move in the direction of perceived visual movement. Vision almost invariably dominated the object-body movement pattern, particularly at values near or below the bodily limen.

(7) More correct and less negative responses were obtained in the deceleration phase of the object-body situation than in the acceleration phase.

(8) The *Ss* varied in the adequacy of their responses to visual, bodily, and visuo-bodily oscillation from situation to situation, and from day to day.

(9) Sensitivity to oscillation of the body without vision showed no improvement with practice. Sensitivity to oscillation of the body when a visual stimulus oscillated with the body showed improvement with practice.

(10) Autokinesis contributed to the perception of movement in oscillation of the body and the object; the amount contributed varied with the individual and with the intensity of the stimuli.

(11) When there were two simple objects in the center of the field of vision, sine-wave oscillation of one resulted in a perception of oscillation of both objects in opposite directions, usually at about the same rate and amplitude.

(12) When one of the two visual objects was oscillated, bodily movement was perceived almost invariably in the direction of movement of the object attended to. There was similarly perceived bodily movement when no actual bodily displacement occurred.

THE INFLUENCE OF HUE ON APPARENT VISUAL MOVEMENT

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Statement of the problem. This investigation was undertaken to determine the influence of hue on apparent visual movement under the condition of successive retinal excitations.[1] The problem of the qualitative and quantitative relations between colored stimulus objects and apparent movement is, in view of the contentions advanced by the Gestalt psychologists, of fundamental importance. The doctrine of the elemental and independent nature of the experience of movement is a critical one for the theory of perception at large; and colored stimuli are crucial to a solution of the matter in question, especially under the condition where the colors of the stimulus members are different.

In this research, accordingly, an attempt has been made to determine how far, if at all, there exists a functional relation between the quality and type of the movement phenomenon and the quality of the spatially and temporally discrete stimulus components. In order to discover the specific effects produced by hue on apparent motion, the intensity and chroma of the colors used in this investigation were held constant; hue, of course varying over a given range.

Historical. Wertheimer furnishes the *locus classicus* for the topic at hand.[2] In his monograph of 1912 he touched upon the effects produced by colored stimuli. Wertheimer gives the impression strongly that one color results in motion as well as another. He has certainly emphasized the so-called 'pure' phi phenomenon and in connection therewith he states that "sehr deutliche, einheitliche Drehung, sinnlich klar zu beschreiben, sichtbar dreht sich der horizontale Streifen ein Stück hinauf, der Vertikale ein Stück in seine Endlage, aber das Ganze ist in Einem, nicht zerstückelte Bewegung, sondern Ganzdrehung klar von *a* nach *b* gesehen, von der Mitte ist bezüglich des sonstigen Optischen zu sagen: da war nichts von Streifen, nichts von rot."[3] Wertheimer shows that partial movement may, however, be the outcome if the stimulus

*Accepted for publication May 13, 1930.

[1]The experiments were performed at the University of Vermont.

[2]Max Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161-265.

[3]*Ibid.*, 222.

members are differently colored, and admits, moreover, that colored motion sometimes occurs. In his consideration of "duale Teilbewegung," Wertheimer says, "der rote Streifen zeigte eine Abwärtsbewegung, ein Stück hinunter von der Anfangslage, der blaue ein Stück in seine Endlage."[4] In another place he states that "der rote lag ruhig da, von einer Lage etwas unter ihm bewegte sich der blaue klar und deutlich in seine Lage hinunter;"[5] and again that "an Stelle des *a* sah man den Strich, bald rot, bald blau, zwischen der Lage *a* und *b* bewegte sich der rote hinüber und herüber. Und ähnlich—komplexer—bei beiderseits gleichzeitiger verschiedener Färbung."[6]

The color technique used by Wertheimer by no means begins to fulfill the requirements of scientific procedure. Wertheimer passes by the phenomenon of colored motion with very brief mention. It is certain that the existence of colored motion would be evidence unfavorable to the thesis Wertheimer and his associates had in mind.

Dimmick,[7] in his study of visual movement and the phi phenomenon, incidentally considered the effect of colored stimulus objects upon seen motion. He states that "movement, optimal in all but a few cases, was reported by all *Os* for all colors and backgrounds when the instructions were to 'characterize.' Under the descriptive attitude there was always noted a gray patch covering the space between the two members. When the backgrounds were white or a light color, the gray was a little darker than medium. On the black field it appeared whitish. We may state, then, with assurance that the gray which is the psychological equivalent of the perceived movement is under our conditions independent, for its quality, of the quality of the members of the stimulus, but shows an influence of the background which has the appearance of a brightness-contrast effect."[8]

Dimmick used four colors, but gives no calibrations for them. He leaves one to guess whether or not stimulus members of different colors were presented; and he has furnished no information bearing upon the quantitative aspects of this part of his research. His general conclusion, however, is completely opposed to the existence of colored motion.

Higginson conducted a tachistoscopic study of the effect upon visual movement of colored stimulus objects.[9] Various combinations of red and green were experimented upon. But there is no information as to the specific hues or the intensities or the chromas of the colors. As a rule, it was found that when the stimulus members were of the same color, optimal movement occurred. When the members were of unlike color there was a decrease in the number of full movements. Dissimilar coloring of the members did not, however, preclude a high degree of integration in the movement experience. Much depends upon the 'set' of the observer. He says that "movement in every case was of a colored

[4] *Ibid.*, 193. [5] *Ibid.*, 209. [6] *Ibid.*, 220.

[7] F. L. Dimmick, An experimental study of visual movement and the phi-phenomenon, this JOURNAL, 31, 1920, 317-332.

[8] *Ibid.*, 332.

[9] G. D. Higginson, The visual apprehension of movement under successive retinal excitations, this JOURNAL, 37, 1926, 63-115.

object, the particular hue depending upon the hue of the stimulus. That is to say, *O* reported that where there was any *relevant* visual filling for the movement it was colored red or green."[10]

Higginson found that, in general, when a line changes in color as it moves across the field the change takes place in a region nearer the second member than the first, although such alteration might be effected in the middle of the movement field. Dissimilar color of the members does not lead to a decrease in the frequency of colored motion. Although, as pointed out above, the calibrations of the two colors used are not given, Higginson evidently considered them to be complementaries, for he states that "where the two lines were complementary in hue, *O* reported once that at the point where the red and green films met, a colorless line was momentarily visible."[11]

It is significant that Higginson failed to find a single case involving intermediate members where the hue of the intermediates was like the color of the unmoving member. The general conclusion is drawn by Higginson that the hue of the seen movement as perceived under his experimental conditions stands in a relation of absolute dependence on the hues of the stimulus objects, and a mere change in the hue of the stimulus object proves sufficient to alter the character of the movement. "Movement," he says, "is not in any sense to be cut away from the hue of the object. It is here that we are unable to agree with Wertheimer."[12] He concludes that "We can state with the highest degree of assurance that the existential correlate of visual movement under discrete retinal excitations with colored stimulus objects cannot be said to be a gray."[13] No quantitative table is presented by Higginson.

Vogt and Grant[14] in their study of apparent visual movement, utilized a fall shutter device for the exposition of the two circular stimulus members. The circular openings were covered by red, green, blue, and yellow filters in various combinations. Nothing is stated as to the specific hues of these colors, and there is not the slightest information concerning the intensities and chromas of the colors in question. No information is presented bearing on the exposure period of each stimulus member; but presumably, since a fall shutter was utilized, the exposition time for the lower circle was somewhat less than for the upper circle. Only 16 reports of full movement out of a total of 1400 expositions were obtained. Three full movements only were reported when the stimulus members were of different colors. The color of the movement experiences reported as 'streaks' seemed to depend largely upon the color of the stimulus components. In a few instances the *Os* reported a 'grayish streak.' Usually, when two different colors were present in the stimulus, the 'streak' was of one color only, resembling the upper or the lower circle. A consistent factor in the movement phenomenon is a streak between the two members. It should be noted that the study of Vogt and Grant contains certain data that are in accord with those of Higginson.

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[10]G. D. Higginson, Apparent visual movement and the *Gestalt*, *J. Exper. Psychol.*, 9, 1926, 244.

[11]*Ibid.*, 248. [12]*Ibid.*, 251. [13]*Ibid.*, 252.

[14]H. G. Vogt and W. Grant, Jr., A study of the phenomenon of apparent movement, this JOURNAL, 38, 1927, 130-133.

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Langfeld,[15] in his research on apparent visual movement with a stationary stimulus, merely touches on motion based upon colored stimuli. He says that good movement was more easily obtainable with blue and yellow than with red, but makes no comment as to green. Langfeld gives no information as to whether the motion was colored or not, he simply states that "movement was obtained with all the colors similar to the movement obtained in the other experiments." [16] One may infer what one pleases from this statement in respect to the occurrence or non-occurrence of colored motion, or the relative frequencies of colored and achromatic movement.

DeSilva made some incidental observations on the influence of colored stimuli.[17] His results in general confirm those of Higginson. In the summary of his protocols he mentions the phenomenon of the "fog overlay" in the region of the "Zwischenraum" and the "sagging" that sometimes takes place in this same region. He reports that the best coloring was obtained with red. DeSilva gives no specifications for his colors except to state that he used red, green, blue, and yellow papers.

The historical survey reveals the fact that there is a need for a special investigation on the problem in question, in which the colors are most carefully determined and controlled. Previous studies have been suggestive, but have evidently failed in the control of the color factor. The aim of the present research is to correct this failure—we intend to use colors that are *adequately controlled*.

Method and Procedure

Apparatus. The apparatus consisted of a pendulum, a vertical projection-screen, and a total reflecting pseudoscope. The screen was 75 cm. high and 50 cm. wide and it was made of dark gray achromatic construction paper (Munsell Black No. 43).

Only one stimulus-pattern was used in the study; it consisted of two horizontal slits, each 6 mm. wide and 24 mm. long, which were cut in the center of the screen. The lower edge of the upper slit was 3 cm. above the upper edge of the lower slit.

The pendulum, which was used for the presentation of the stimulus-colors, was placed just to the rear of the screen. Its lower end was equipped with a card-holder that carried the stimulus colors. The pseudoscope was mounted before the slits with its eye-piece 153 cm. from them. It was used to eliminate visual distractions and to restrict eye-movements.

The screen was illuminated by two 100-w. Daylite Mazda lamps which were placed immediately to the right and left of the pseudoscope and on a level with it. The pseudoscope was provided with an adjustable chin-rest.

[15] H. S. Langfeld, Apparent visual movement with a stationary stimulus, this Journal, 39, 1927, 343-355.

[16] *Ibid.*, 353.

[17] H. R. DeSilva, Kinematographic movement of parallel lines, *J. Gen. Psychol.*, 1, 1928, 550-577.

Stimulus-cards. Nineteen stimulus-cards were prepared by pasting rectangles of colored paper upon cardboards, which were of a size to fit the pendulum card-holder, and which had been faced with the same dark gray paper that was used in the construction of the projection-screen. One, a practice-card, was made of white (Munsell construction paper No. 41); and the others, the experimental cards, were made of the following colors: (1) yellow–yellow, (2) green–green, (3) blue–blue, (4) red–red, (5) yellow–yellow, (6) blue–blue, (7) red–red, (8) green–green, (9) red–yellow, (10) yellow–green, (11) red–blue, (12) green–blue, (13) red–green, (14) yellow–blue, (15) red purple–green, (16) blue–yellow red, (17) green yellow–purple, and (18) purple blue–yellow. The colored papers used in the first four stimulus-cards (1–4) were Milton Bradley,[18] those in the remaining cards were Munsell.[19] The color pairs in the last four stimulus-cards (15–18) were complementary. The color first named for every card was presented in the upper slit, and the one secondly named in the lower slit.

Exposure-time. The horizontal dimensions and the spatial relations of the colored papers that were pasted upon the stimulus-cards were carefully and accurately determined. When the pendulum was released from a constant height a color appeared in the upper slit for a period of 175 σ and then, 15σ later, the same or a different color appeared in the lower slit for a period of 175 σ.[20] The pendulum was permitted to swing but once in every exposure. The objective movement was from the *O*'s right to left, hence the apparent movement in the pseudoscope was from the *O*'s left to right.

Observers. There were 5 *O*s in the experiment: Miss Marian Baldwin (*B*), Miss Anne Edmunds (*Ed*) Miss Marian Everest (*Ev*), Mrs. Paul C. Squires

[18]The Munsell Co. calibrated these Milton Bradley papers for us in terms of their own scale, and found that the red was 3 R, 2.8/8.2; that the yellow was 5 Y, 8.1/9.0; that the green was 3 G, 5.0/8.5; and that the blue was 6 B, 3.3/11.5. This notation means that the blue, for example, had a position on the band of hues indicated in the Munsell scheme by the number 6 (the so-called typical hue being located at 5), that the value or intensity was 3.3, and that the chroma was 11.5.

[19]The hue number of each of the Munsell colors was 5, the value or intensity was 5, and the chroma was 6. An intensity of 5 is the mid value in the Munsell scale, and a chroma of 10 is the highest possible value. A chroma of 6 was the maximum value that could be obtained for the various colors at the intensity level of 5. The Munsell papers are comparable in respect to the influence of hue, since the same intensity and the same chroma are possessed by all of these colors.

[20]Since the slits were not filled with color instantaneously—the pendulum carried the stimulus-colors in and out of the slits horizontally—the temporal values stated above require definition. The exposure period of 175 σ means that this period elapsed from the moment a color (traveling from right to left in the slit) had its left edge exactly even with the right edge of the slit, to the moment the color had just disappeared from the left edge of the slit. The temporal interval of 15 σ is defined as that period elapsing from the moment the stimulus-color disappeared from the left of the upper slit to the moment it reappeared in the right of the lower.

Acknowledgment is gratefully made to Professor R. M. Holmes of the Department of Physics, University of Vermont, for performing the mathematical computations involved in the determination of the horizontal lengths of the colored papers so as to obtain these temporal values and for his advice and assistance in the construction of the pendulum.

(*S*), and the writer (*W*). Of these the first four were naïve in respect to the phenomenon of apparent visual movement and untrained in introspection.

Instructions. The following instructions were given the *O*s.

"Fixate the field in front of you in whatever manner may be from time to time directed and do not allow your gaze to wander. When you fixate be careful to distribute your attention as evenly as possible over the field that just includes the two rectangles in the screen; keep your attention as maximal and as constant as you can. Assume a receptive and totalizing attitude. Make the descriptions of what you observe photographic, not figurative. Eliminate from your descriptions all assumptions and inferences. You may construct diagrams whenever you feel the need, or E expresses the need, of supplementing your verbal descriptions."

Procedure. After the instructions had been presented the pseudoscope was adjusted by the *O* so that the horizontal slits in the screen were in the middle of the pseudoscopic field. Then the chin rest was adjusted; and then, at a given signal, the pendulum was released by means of a noiseless trigger and the stimulus-card was swung behind the slits. The pendulum was caught by *E* at the height of its excursion to the left and held in that position until a cardboard shutter had been interposed between the pendulum and the slits; then the pendulum was brought back to the original position and reset for the next experiment without exposing the colors to the *O*. The method of single exposures was used throughout the study.

The white practice-card was used 10 times at the beginning of every experimental period. This series of exposures was introduced so as to adapt the *O* to the experience of movement and it made, we believe, for the stabilization of the results. At the conclusion of the practice-series the next occurring stimulus-card was inserted into the pendulum card-holder and 5 preliminary exposures were made. During these exposures no introspective reports were taken. These exposures merely served further to adapt the *O* to the experience of movement and to acquaint him with the quality of the stimulus-card. At the completion of these preliminaries the stimulus-card was exposed 15 times, after every one of which an introspective report was dictated by *O* and recorded by *E*. Three different points of fixation were used in this, the experimental series proper. In the first 5 exposures of this series a point midway between the two slits was fixated; in the second 5 a point 7.5 mm. below the lower edge of the upper slit; and in the third 5 a point 7.5 mm. above the upper edge of the lower slit. These points of fixation will be referred to hereafter as central, upper, and lower.

Two series of experiments, Series A and B, were conducted. In Series A the stimulus-cards were presented in the order (1) to (18). In Series B only the last four of the stimulus-cards were used and these were presented in the order (16), (15), (18), and (17). Series B was conducted for the purpose of checking the *O*'s reports in Series A, and also for their own sake. We wished to make an intensive study of the effect of complementary colors. Every *O* gave 270 reports in Series A, and 60 in Series B.

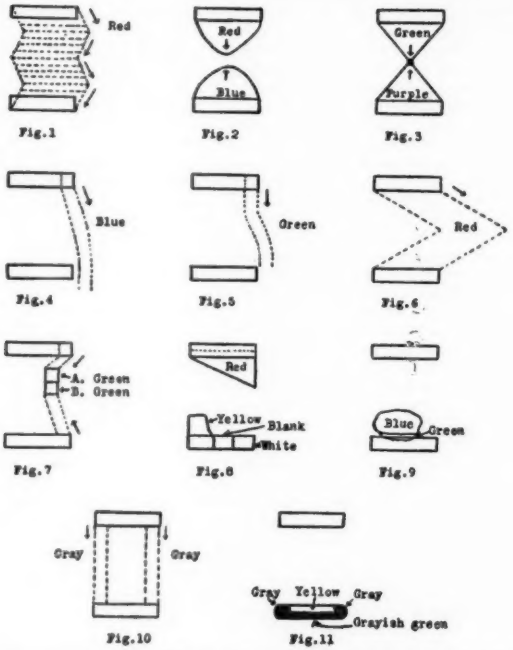
The experiments were conducted in a room from which all the light, except that furnished by the two 100-w. lamps, was excluded.

Results

Qualitative. Under this head we present a few samples from the protocols of the *O*s. The notation prefixed to each is to be understood as follows: the first member of the notation refers to

the series, the second to the stimulus-card, and the third, the italic numeral, to its position within the group of 15 reports.

Ed [A-3-*2*]: "A blue line appeared at the top position and moved down to the bottom position with medium speed. The movement was smooth and continuous, but there was no content in the field included by the upper and lower positions of the moving line. There was motion without anything moving.



FIGS. 1-11. DIAGRAMS SHOWING THE RELATION OF COLOR AND MOVEMENT REPORTED BY *Ev*, *S* and *B*.

It seemed as though the line descended behind the screen and was squeezed up against the rear of the screen, being all the while concealed by the screen. The blue line, when it appeared in its upper and lower positions, was on exactly the same level as the screen."

W [A-18-*6*]: "A blue rectangle about the size of a slit moved rapidly down from the top position to just below the center of the field. The instant the blue line got there a yellow line appeared just above the center of the field. This yellow line poised for an instant and then shot down to the bottom position, passing over the stationary blue line. As the yellow line passed over the blue one the latter disappeared just for an instant and then reappeared as soon as the yellow line had passed over it. The blue line disappeared just a moment

before the yellow line attained bottom position. The speed of the blue line was medium, that of the yellow one very rapid. The pattern of the motion greatly surprised me. The moving blue line was of film order, while the yellow line was at every instant of a surface character."

Ev [A-7-*3*]: "A red line, having very narrow width, moved rapidly from top to bottom position. The line 'flopped' inward and outward several times in the course of its descent. After this motion had been completed a pink film emerged from the background and spread over the whole field, by which I mean, as usual, that part of the total field which is included by the two terminal positions" (see Fig. 1).

[A-11-*2*]: "I perceived motion without anything moving, the motion being from top to bottom of the field. After the completion of motion a red film began to move downward over the field and, after the beginning of this film motion, a blue film began to ascend from the bottom position. The motion of each film was extremely sluggish. These films finally almost met at the center of the field. During the film movements there were distinctly visible a red line having the width of a slit and situated at the top of the field in the position of the upper slit, and a blue line in the lower slit. These lines were of film character" (see Fig. 2).

[A-17-*1*]: "Full motion downward with nothing moving. After this motion was completed a green film moved downward from the top and at the same time a purple film ascended from the bottom of the field. These film movements occurred simultaneously and each film assumed triangular form with vertex at the center of the field. Purple and green lines of surface character were at the bottom and the top of the field respectively during the movements of the films. These lines disappeared at one and the same time and the films were simultaneously present in the field for a brief interval after these terminal colored lines had vanished" (see Fig. 3).

[B-15-*10*]: "A purple line in the top position and a green line at the bottom of the field made their appearance simultaneously. The green line oscillated toward the right and then toward the left, horizontally, for at least three complete oscillations."

[B-18-*15*]: "Very rapid motion, nothing moving, from upper right to lower left hand corner of the field. Can not say much about it. It was hard to get at, like a rapidly moving and highly indefinite shadow, and yet whatever was moving did not have the quality of any shadow. I am sure, however, that, absurd as it may seem, this motion without content never lost contact during its course with the upper right hand corner of the field. There was motion not only in the locus of the diagonal but also outside of this locus. However, the motion outside of the locus in question was not so marked; this 'secondary' motion was in the same direction as the 'primary' motion."

S [A-6-*11*]: "A blue line leaped in an arc-like manner from the top to the bottom of the field. The moving line had the dimensions of a slit and its color was of surface character."

[A-6-*15*]: "A blue line moved with moderate rapidity from top to bottom position. The blue line in its downward motion stood out from the background throughout its entire course."

[A-8-*7*]: "A green line moved from the top position one-half way to the bottom of the field with a slow motion. Before this line had attained the half-way mark a green line shot up from the bottom position and merged with the first line at the half-way position. When the two lines merged, the shape of the area of 'mergence' was indefinite."

[A-18-*11*]: "There was motion from top to bottom of the field; I should say the motion extended to below the bottom position. Rate of motion very rapid. A blue piece, of surface character, grew out of the upper slit at its right hand end and extended downward just as I have sketched it. The entire area indicated by the dotted lines was finally filled with blue. While this motion was

taking place the upper slit was filled with blue and shortly after the motion started the lower slit was filled with yellow. The upper and lower colored rectangles and colored path of motion disappeared at one and the same time." (Fig. 4).

[B-17-*11*]: "Motion similar to the one I recently sketched," *i.e.* [A-18-*11*]. "The path of motion was green, and the motion extended to the upper edge of the lower slit. In this case the motion was very slow, however, reminding me of a slow moving picture" (see Fig. 5).

B [A-6-*9*]: "A blue line *ascended* from bottom to top of the field and stood out in front of the background as it moved upward. It was in front of the level of the background, not merely in the field between the two terminal positions, but also at the lower and upper positions. A film followed in the wake of this line, the film being of a lighter blue than the blue of the moving line. The line had the same width as a slit and its blue color was surface character. The motion was of medium speed."

[A-7-*5*]: "A red line appeared at the top position and then moved downward in a 'V-like' path, but maintained the horizontal position all the while. The speed of motion was above average. The line, as soon as it began to move, lost all hue and possessed only the vaguest form. It was as though something were hitched to the back of the line and moved it in the manner I have indicated in the sketch" (see Fig. 6).

[A-8-*2*]: "The upper slit was filled with green and then a piece of this line detached itself from the line at the right hand side and moved downward, and slightly toward the left, to the position I have designated as 'A' in the sketch. After this motion got under way a similar piece broke off from the corresponding part of a green line situated at the bottom of the field and moved upward and slightly toward the left until the two pieces just touched each other. These two pieces disappeared simultaneously. I should note that the green line at the bottom of the field appeared after the line at the upper part of the field had appeared. The upper and lower lines vanished simultaneously. Speed of motion was medium. The detached and moving pieces were of the same width as a slit and about one-quarter the length of a slit. After the two pieces broke off from the upper and lower lines the gaps were immediately replaced by green filling so that the upper and lower lines were thoroughly completed" (see Fig. 7).

[A-9-*9*]: "A red line appeared in the top slit but not up against the upper edge of the slit and grew out of the lower edge of this slit so as to form a surface of triangular nature as shown in my sketch. After this had taken place, but while the triangle was still present, a yellow filling poured into the left hand half of the lower slit from above as I have indicated in the sketch; after the yellow had poured in, white appeared in the right hand part of the lower slit. The portion of the slit located between the yellow and the white was blank, that is to say, contained nothing at all. The motion of the red and the yellow was slow. The white did not move: it was simply there and disappeared as though blotted out" (see Fig. 8).

[A-13-*5*]: "A red line appeared at top of field and then motion downward occurred, but do not know what moved. There was a blue blotch just below the center of the field which was of surface character. On the lower edge of the blotch was a green band, but this green band extended into the lower slit only a very little ways. The blotch was simply there and did not move" (see Fig. 9).

[A-18-*11*]: "A blue line appeared first at the top position and then there was a yellow line at the bottom of the field. After both lines were present streams of gray moved at medium speed from the top to the bottom" (see Fig. 10).

[A-18-*12*]: "A blue line appeared at the top of the field and then a hollow cylinder appeared at the bottom position. In my sketch I have indicated the appearance of the cylinder. That part of the cylinder marked yellow means that a middle section on the upper surface of the cylinder was missing. The yellow designates the color of the inside surface of the cylinder which was open to inspection. A very strange thing" (see Fig. 11).

The 19 protocols reproduced above, constitute about 1% of the total number of reports. They will suffice, however, to demonstrate the large individual differences among the 5 *Os*.

Full movement was seen by *Ed* at the first exposure. It was "striking," it "thrust itself" upon her, and it was "very energetic." In not a single instance did *Ed* fail to perceive movement of the full type. She reported variations only in speed and smoothness, which are of no significance to this study. At no time were the colored lines at the upper and lower terminal positions seen as lying behind the background; they were always in the same plane as the background. *Ed* always perceived *one* line descending from top to bottom of the field. Even in those experiments in which the color of the stimulus-members differed she saw but one line in motion; and this line underwent an instantaneous alteration in hue at the moment it reached the bottom of the field. Her reports of movement came immediately and without a trace of uncertainty or hesitation.

At the close of the experimental series, in order to test her reports for possibly artifactal experiences of movement, she was given instructions to refuse to see movement. Though she did her best to destroy the phenomenon, it persisted in all its previous strength. There is no doubt whatever that she, throughout the entire course of the experiment, regarded the movement as real and was in no way aware of its illusory nature.

One peculiarity in the experiences of *Ed* was that no content was actually observed to be in the field between the upper and lower positions of the moving line, and yet she says that motion was clearly and unequivocally present. Time and again she made the remark that the line seemed to be moving down the field "behind the background and squeezed tightly up against it." She volunteered that this statement sounded absurd, "but it is a fact, just the same." This constitutes a problem which will receive consideration later on under the proper heading.

W was quite consistent in perceiving the moving line as possessing approximately the width of a slit. The color content of the moving line was as a rule highly definite. A few cases of pure phi were met with. In these the movement was perceptual, not the mere meaning of motion. There was evident a tendency for movement to approach more and more closely the status of pure phi as its speed increased. The speed, however, was not always constant for a given exposure, it varied considerably at times; for example, the line might drop rapidly to the middle of the field, then pause for just an instant, and then continue its course slowly to the bottom of the field. The character of the color of the moving line underwent alterations at times, changing fron film to surface or vice versa. *W* was especially impressed with his observation quoted above; it illustrates forcibly the possible complications in the experience of movement.

Ev was subject to considerable variability in her experiences of movement. Typically it was rapid and associated with film effects which waxed and waned in a variety of spatial and temporal patterns. A "flashy brightness," which is doubtlessly Dimmick's "gray flash," was described by her a few times. She also reported instances of tridimensional movement and of oscillatory (see B-15-*10* above) and diagonal (B-18-*15*) movement. She experienced pure

phi a number of times, and also mere meanings of movement. The width of the moving line was usually very narrow and many times had mere length. At other times, however, it was the width of the slits and vividly colored.

S drew no distinction whatever between 'real' and 'illusory' movement. All the movements were 'real' to her, and she was much surprised to learn, at the close of the experiment, the nature of the apparatus. This *O* always perceived the colored lines in the slits as being on the same plane or in front of the background, but never behind it. She perceived full movement as follows: the line at the top of the field moved down to the bottom, where it was replaced by another line. That is to say, she perceived *two* lines always involved in the phenomenon of full movement—not one line moving which changed color when it arrived at the bottom of the field. The slower the motion of the line the wider did it appear to be. The color of a line remained unaltered during movement, and it was consistently of surface character. Wherever the moving line happened to be at any given instant there the background was obscured. Color was always limited to the area of the moving line and did not spread outside of this region. It should be noted, however, that in the case of bimembral movement, at the moment when the two lines were merged one into the other, the surface character of the colors changed into something that looked neither like surface nor film, but like something representing a half-way stage between the two.

S could always easily distinguish width of line at every instant of its motion. Higher speeds tended to carry with them a "standing out" of the moving line from the background. Sometimes the moving line was shortened considerably during transit. For instance, in A-1-*12* the report was to the effect that the line shortened by one-half and symmetrically in respect to the vertical boundaries of the field during the entire course of motion. That is, the change in length of the line was a continuous one. On the other hand, in A-1-*15*, the line shortened by three-quarters of its length and symmetrically at the instant it arrived at the bottom of the field. Sometimes, the moving line grew in length from top to bottom of the field, until at the bottom it had assumed the same area as the slit. *S* also observed tridimensional movement. For instance, in A-2-*11* the line, when it was two thirds way down the field, "hurdled" the rest of the distance in an arc-like manner to the bottom, bowing out considerably toward the *O*. In A-4-*9* the line itself as it moved was tri-dimensional. "There was so much color in the line that the color seemed to puff out all the way down. It was neither transparent nor opaque, but a mixture of the two —a very peculiar experience."

The complementary colors gave nothing but full motion or the bow-motion. For the latter see Figs. 4 and 5. Under the supplementary instructions, "you are to refuse to see motion," great difficulty was experienced by this *O* in disintegrating the motion. Though she was partially successful she never succeeded in completely destroying it. The only way in which she could break down the movement was to think of the lines as parallel. She never perceived a case of pure phi, or motion as meaning. All motion had concrete and definite content.

B manifests an astounding array of complicated and shifting phenomena. The reports are so variegated that it has been virtually impossible to analyze

them according to the conventional rubrics. A few general aspects only are here presented in a summary manner, together with certain exceptional and bizarre features.

Colored movement of some order or other was the rule in *B*'s case. Films frequently trailed in the wake of the moving line. Movement was usually of medium speed. Pure phi did occur, but was very rare. In some experiments the background became transparent and the colored line moved in a plane behind the background; sometimes the background disappeared, strange to say. Pieces of the line or lines were observed to detach themselves amd move downward and upward, as in A-8-*2*. Rainbow effects and compound lines of various orders were observed. A-9-*9* is an example of a very complicated movement phenomenon. In some experiments the moving line was observed to turn completely over as it came out of its slit. The most unusual observation made by *B* is no doubt the 'cylinder effect' in A-18-*12*. In one instance there was full motion of a highly colored line down to the bottom of the field and all the way back to its original position. The gray streams in A-18-*11* are evidently based on the strictly complementary relations between the stimulus-members. Scores of other observations could be produced here to bear out the highly variable and complex nature of *B*'s responses to the apparent movement situation. But limitation of space precludes a further account of these. Motion was always real to *B*. There was a firm belief entertained by this *O* that the colored lines were actually moved by machinery. At the close of the experiment, when shown the nature of the apparatus, *B* had difficulty in convincing herself that no motion in the physical sense had at any time occurred.

Quantitative. The following tables consolidate the results based upon the Munsell colors only, for the reason that these colors

TABLE I

SHOWING FOR EVERY *O* AND FOR EACH EXPERIMENTAL CONDITION THE NUMBER OF TIMES THAT THE VARIOUS PHENOMENA WERE REPORTED

(Con. = experimental condition; F. = full movement, perceptual; F(M). = full movement, meaning; PP. = pure phi; Un. = unimembral movement; Bi. = bimembral movement; D. = delta movement; Di. = diagonal movement; Bw. = bow movement; In. = intramembral movement; GF. = gray flash; P. = partial movement; Suc. = successive stage; and Sim. = simultaneous stage)

Con.	*O*	F.	F(M).	PP.	Un.	Bi.	D.	Di.	Bw.	In.	GF.	P.	Suc.	Sim.
One Color	*W*	48	4	2	0	2	0	0	0	0	1	2	1	0
(60 experi-	*Ev*	37	12	3	0	0	2	2	0	0	0	0	3	1
ments)	*S*	54	0	0	0	1	0	0	5	0	0	0	0	0
	B	11	7	2	4	3	8	0	0	9	0	3	7	7
Two Colors	*W*	61	15	5	3	0	0	0	0	0	3	0	2	1
Non-comple-	*Ev*	40	17	2	4	0	1	2	0	0	4	2	18	2
mentary (90	*S*	66	0	0	6	17	0	0	0	0	0	0	1	0
experiments)	*B*	7	2	1	14	16	6	3	0	12	0	23	19	6
Two Colors	*W*	97	10	3	2	0	0	0	0	0	2	0	3	1
Complemen-	*Ev*	41	16	9	3	0	2	4	0	4	4	0	16	21
tary (120	*S*	117	0	0	0	0	0	0	3	0	0	0	0	0
experiments)	*B*	22	3	3	16	6	5	1	0	5	0	16	47	8
Total		601	86	30	52	45	24	12	8	30	14	46	117	47

were rigidly controlled, while the Bradley colors were really non-comparable. It will at once be noticed that *Ed* has no place in the tables. The reason for this is that she never reported motion except as a meaning only, with no visual content as such present.

Every *O* had, as Table I shows, 60 experiments with the lines being of one and the same color, 90 experiments with the lines being of different and of non-complementary colors, and 120 experiments with the lines being of complementary colors. Movement of one kind or another was reported in approximately 83% of the experiments, and colored movement was reported in 72% of these.

TABLE II

SHOWING FOR EVERY *O* THE NUMBER OF TIMES THAT VARIOUS PHENOMENA OF MOVEMENT WERE REPORTED AND THE POSITIONS WITHIN THE FIELD AT WHICH HUE CHANGES OCCURRED.

(Col. = colored movement; Tri. = tridimensional movement; Inter. = intermediate; Comp. = 'complementary gray'; C = central fixation; U = upper fixation; L = lower fixation; Ave. = average without respect to fixation.)

O	Col.	Tri.	Inter.	Compl.	Fixation			Ave.
					C	U	L	
W	215	2	2	0	.7	.8	.88	.80
Ev	127	7	2	7	.5	.6	.6	.56
S	269	8	0	0	.9	.9	.98	.96
B	173	24	21	32	.9	.9	.9	.90

The figures under 'Fixation' in Table II are to be understood as follows: .56 for example, means that the hue of the moving line changed on the average about one-half way down the field; the figure .9 means that it changed nine-tenths the way down, and so on. Average region of change for all the *O*s was .80.

DISCUSSION OF RESULTS

A survey of the data reveals the fact that movement of some sort occurred in the great majority of the experiments, and that the movement was colored in most instances.

The foregoing account has served to show the marked individual differences encountered in this research. A few remarks will at this point be made concerning the various *O*s.

Ed evidently experienced motion of a quasi-perceptual order. She reports motion without anything moving. But this does not refer to the pure phi-phenomenon, which is an experience of the genuinely perceptual order. Inquiry developed the fact that, for her, motion as obtained under the conditions of the experiment represented a half-way stage between visible motion and motion as a meaning only. The hypothesis is here suggested that she was using the term 'see' in an equivocal sense without realizing this to be the fact, although toward the close of the experiment she seemed to entertain a vague suspicion that there lurked an absurdity in her statement that she could see a line descending *behind* the screen. It is probable that her experience of motion was based primarily upon the mechanics of general bodily kinesthesis, involving certain empathic factors in the more specified sense of that term.

Whether this *O* would report movement of other orders under different experimental conditions would constitute an interesting problem in itself.

S is noteworthy for the remarkable consistency with which highly colored motion of sharply defined lines took place, the colors being of surface character.

Ev drew a careful distinction between the pure phi-phenomenon and movement as mere meaning. Pure phi has actual perceptual content of the visible order, but its tenuousness is of the *n*th degree.

No further remarks need be made concerning *W*, except that he also has no hesitation in drawing a distinction between the pure phi and movement as meaning, although it must be admitted that the task of drawing this distinction is a most arduous one, as a rule.

B evidences a most bewildering variety, complexity and 'unreliability' of response. As a matter of fact, many of her reactions are scarcely to be subsumed under any previously known categories, and a fully satisfactory classification under conventional rubrics is impossible. The tabular summary that has been made for *B* represents, however, the best that could be done under all the circumstances.

On the whole, the movement phenomenon is to be regarded as anything but simple. Frequently it may be extremely intricate, as particularly in *B*'s case.

The 'gray flash' encountered in this investigation does not seem to require the presence of the 'process' attitude for its occurrence, since introspective report conclusively showed the presence of the totalizing attitude. This does not signify that a deliberate assumption of the 'process' attitude would not have resulted in a larger number of 'gray flashes:' this is matter for separate investigation. The 'gray flash' was simply there; it did not move, and was of very brief duration. Dimmick and Sanders refer to it as a "flickering bulky-film."[21] Our *O*s did not perceive it as flickering, but as flashing. Five chief characteristics are ascribed by Dimmick and Sanders to this 'gray.' Our observations agree with all but the third characteristic, wherein the 'gray' is described as "flickering." So far, however, as regarding this 'gray' as the existential correlate of visible movement is concerned, the results of the present study are self-evident. The 'gray flash' occurs in approximately 1% of all cases, but is thus merely one of the less frequently observed phenomena conditioned by the successive exposition of two colored stimulus members—nothing more.

The average value, for all *O*s, as to the region in which a moving line changes its hue, is four-fifths way down the field.

[21] F. L. Dimmick and R. S. Sanders, Some conditions of the perception of visible movement, this JOURNAL, 41, 1929, 607-616.

This is in substantial agreement with the findings of Higginson[22] that the change takes place nearer the second member.[22] Individual differences, however, were marked; *Ev*, for example, reported the change near the center, and sometimes a short distance from the first stimulus member; these cases , it should be stated, are rare.

In respect to the complementary color pairs, 2 *O*s only obtained the complementary effect. The total number of instances in which this effect occurred is 39, and 27 of these appear under succession and simultaneity. *S* did not observe the effect in question; she obtained a much higher frequency, practically 100%, for full motion with the complementary than with the different but non-complementary color pairs.

Motion of the 'complementary gray' kind has hence been found to possess a negligible frequency. It would seem highly probable that the complementary effect obtained in this research is the result of slips in fixation, through which the after-image of one stimulus member is superimposed upon the other member.

The present study would suggest eye-movements and the after-image to possess partially explanatory values. For instance, marked differences were noted from time to time in the speed at which a moving line passed over various parts of the field. A line would shoot down from the top of the field, pause perhaps at the center, and then complete the motion in a sluggish manner to the bottom. Then, there were observed vibrations of a line in various directions, rapidly and over a narrow range. Such phenomena would certainly seem to receive the best explanation in terms of eye-movements. Guilford and Helson,[23] in their recent admirable research on eye movements, have arraigned most severely the eye-movement theory. Suffice it to say here, however, without entering into any extended discussion of the matter, that these investigators have for one thing apparently confused physical time with *duration* (hence giving expression to a gross form of the 'constancy hypothesis'), and for another thing have advanced an argument scientifically unjustifiable; namely, that a theory having partially explanatory value should be totally rejected. In respect to the rôle of the after-image, this would most assuredly

[22] G. D. Higginson, *op. cit.*, this JOURNAL, 37, 1926, 114.
[23] J. P. Guilford and H. Helson, Eye-movements and the phi-phenomenon, this JOURNAL, 41, 1929, 595-606.

seem to possess great importance in such phenomena as are involved in film effects, for instance; and retinal streaming no doubt plays some part or other in these effects.

Summary and Conclusions

The present investigation has been devoted to the problem of the influence of hue on apparent visual motion. Five *O*s were used, 4 of whom were completely naïve in respect to the phenomena of apparent movement. The main conclusions derived are as follows.

(1) Movement was reported in 83% of the exposures, and colored movement was found in 72% of them.

(2) The 'goodness' of movement may be high or perfect, even though the stimulus members are of different hue.

(3) There exists a higher frequency for unimembral, bimembral and various kinds of partial motion when the stimulus members differ in color. This frequency is higher for the non-complementary than for the complementary color pairs.

(4) No significant correlation was found to obtain between hue or hue combination and pattern of movement.

(5) The 'reality' of movement under the conditions of this experiment was highly compulsory.

(6) A most remarkable complexity and variability of phenomena may be obtained under one and the same stimulus-conditions.

(7) From conclusion (6) there follows the proposition that the stimulus-pattern is of secondary importance in the determination of movement phenomena. All the evidence converges, 'beyond a reasonable doubt,' to the conclusion that mental set or attitude is of primary importance.

(8) The 'gray flash' is in no way the essence of apparent movement, when this movement is grounded upon colored stimulus members.

(9) This study utilized colors which were strictly controlled. The colors were derived from the Munsell Color System.

(10) If the Gestalt-Theorie in the field of apparent visual movement is presumed to depend upon the proposition that quality of movement is "cut away from," or relatively independent of, the quality of the stimulus, then this theory is without firm foundation. All the facts tend to demonstrate an extremely high degree of functional interrelation between quality of movement and quality of the stimulus configuration.

THE TIME OF JUDGMENT IN PSYCHOMETRIC MEASURES

By W. N. Kellogg, Indiana University

In previous experiments upon the methodology of psychophysics,[1] the problem of the time consumed in making different types of judgments was continually arising. What was the relationship between judgment-time and the number of categories of judgment permitted the observer? Could time be taken as a valid criterion of introspective estimates of confidence? How did judgment-time vary with the objective difference between pairs of stimuli judged? Was it possible that a reasonably definite and predictable connection existed between time and stimuli from which psychometric time curves analogous to the $\Phi(\gamma)$ functions could be determined? Since the literature yielded no answers to some of these questions and but incomplete ones to others, an experimental investigation was undertaken in the attempt to throw light upon the part played by judgment-time in certain of the accepted psychophysical procedures.

Historical

The problem of time measurement of discrimination and judgment processes has an extensive historical background if one considers that the reaction-time experiments are relevant. In view of the fact, however, that these for the most part have been concerned with the measurement of the *greatest possible* speed of response rather than with the time of judgment where the emphasis was not upon speed, they appear to us to be of secondary importance. Two or three reaction-time studies, related perhaps closest to the present problem, are consequently all that will be discussed here.[2]

(*a*) *Reaction Time.* As early as 1885 Merkel completed an experiment in which the subject was required to react first with one reaction-key to a single stimulus, then with one of two keys to either of two stimuli, and similarly with

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[1]This is the third of a series of studies in psychophysics by the writer. The first two, An experimental comparison of psychophysical methods (1929) and An experimental evaluation of equality judgments in psychophysics (1930) were published as monographs in the Arch. of Psychol. (nos. 106 and 112)

[2]For a general review of the literature on reaction-time the reader is referred to R. S. Woodworth and A. T. Poffenberger, *Textbook of Experimental Psychology*, 1920.

one of three, four, five, six and so on up to ten reaction keys (one for each finger on each hand), to one of a corresponding number of stimuli.[3] The keys were successively numbered, and the stimuli were the numbers themselves. When a given number was shown, the subject was to react with the appropriate key and with no others—always with a foreknowledge of the number of alternatives for which he had to be prepared. Merkel discovered that the greater the number of alternatives the slower the reaction-time, so that a perfect progression existed between the number of possible reactions and the time consumed in reacting.

Henmon's investigations of the discriminative reaction-time also bear upon the present problem.[4] He found, using two stimuli and two reaction-keys, that the smaller the objective difference between the stimuli the slower the discriminative reaction-time. The same relationship obtained regardless of the sense modality in which the stimuli were presented. In a later study he considered the question of the time consumed in making correct as against incorrect discriminations. From the results of his second paper, he concluded that "the time of wrong judgments is on the average longer than the time of right judgments,"and that "the time of judgment increases uniformly as the confidence decreases."[5] A further result of some importance was that the dispersions of the distributions of wrong times tended to be greater than those of the right ones.

One may well ask whether the relationships which Merkel and Henmon found when the subject was under pressure to react as quickly as possible, obtain also during the normal course of any series of judgments where the judgment itself and not the time factor is the primary consideration.

(*b*) *Judgment-times where speed is not the main objective.*[6] Martin and Müller, in their classic experiments with lifted weights, were the first investigators who attempted to record the time of judgment in the normal course of a series of regular psychometric determinations.[7] Their method was, however, crude and inaccurate. Times were obtained by counting the beats of a metronome during the interval which started when the second or comparison weight of a pair had been set down and ended when the *O* pronounced judgment. The beats were 0.7 sec. apart. The investigators concluded from their results that many unknown factors influence the time of judgment and that, particularly if such impressions as doubtful or undecided play a part, the judgment is likely to be longer.

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[3]Julius Merkel, Die zeitlichen Verhältnisse der Willensthätigkeit, *Phil. Stud.*, 2, 1885, 73-127.

[4]V. A. C. Henmon, The time of perception as a measure of differences in sensations, *Arch. Philos., Psychol., & Sci. Methods*, 1906, (no. 8).

[5]V. A. C. Henmon, The relation of the time of a judgment to accuracy, *Psychol. Rev.*, 18, 1911, 195.

[6]For more complete reviews of the literature on psychophysics the reader is referred to E. B. Titchener, *Experimental Psychology, Instructor's Manual*, Vol. II, Part II, 1905; R. S. Woodworth and A. T. Poffenberger, *op. cit.*, 1920, 240 ff; G. J. Rich, Psycho-physical measurement methods, *Psychol. Bull.*, 22, 1925, 613-648; and W. N. Kellogg, An experimental comparison of psychophysical methods, *Arch. Psychol.*, 1929, (no. 106), 5-16.

[7]L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit*, 1899, 1-96.

A careful introspective study by Fernberger confirms in the main the findings of Martin and Müller. Although he had no objective measure of time, Fernberger noticed "that the equality judgments were seldom so immediate as the difference judgments and they were usually given with a lower degree of assurance."[8]

Two more recent attempts to relate the time of judgment to confidence estimates may be mentioned in the experiments of Seward[9] and Rosenbloom.[10] The former investigator recorded the time consumed in recognizing cards on which complex patterns were printed in a variety of colors. Correlations of from 0.37 to 0.81 between confidence-estimates and recognition-times were obtained. Seward concludes that "the more accurate response is quicker and more confident than the more inaccurate regardless of whether it is positive or negative."

Rosenbloom undertook an ingenious experiment in the recognition of 'closed' and 'open' *Gestalt* form-patterns presented *tactually*. He found the recognition of the 'closed' figures was accompanied by a greater degree of accuracy, a higher confidence, and a faster recognition (or judgment) time than the 'open' figures.

The experimental evidence which is most directly related to the present problem, however, was advanced by George.[11] This investigator had 3 *Os* who judged sound intensities and two more who judged visual extents. The times of judgment were recorded on a kymograph by means of a voice key and electric tuning fork.

George used the method of constant stimuli although in addition to the usual categories of judgment he included a number of others, *e.g.* 'doubtful,' 'no difference,' and various types of 'or' classes as 'greater-or-equal,' 'less or equal,' and so on. He was interested specifically in discovering the function and importance of the doubtful class. As far as times are concerned he found the average judgment for the category 'less' required 0.88 sec., for the category 'greater' 0.92 sec., for the category 'equal' 1.10 sec., and for the category 'doubtful' 1.42 sec. The long times for the doubtful cases he thinks probably indicate "that the judgment doubtful is of the reflection type."[12] His general conclusion is that doubtful or uncertain judgments should be excluded from the psychophysical measurement methods.

Plan of Present Experiment

It will be noted that with few exceptions those investigators seeking to measure the time consumed between the presentation of the stimuli and the rendering of the judgment have employed apparatus or methods which made it impossible to keep the purpose of the experiments from the observer. This, it seems to us, largely negates the value of the findings, since the *O* judging

[8]S. W. Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, (no. 6), 155.

[9]G. H. Seward, Recognition time as a measure of confidence, *Arch. Psychol.*, 1928, (no. 99), 1-54.

[10]B. L. Rosenbloom, Configurational perception of tactual stimuli, this Journal, 41, 1929, 87-90.

[11]S. S. George, Attitude in relation to the psychophysical judgment, this Journal, 28, 1917, 1-37.

[12]*Ibid.*, 14.

with the awareness that the time he consumes is being checked, certainly possesses a different attitude toward the whole experiment than the *O* who has his entire attention upon making the judgments themselves. Such knowledge must perforce introduce spurious factors which not only act as secondary distractions but which probably shorten the judgment-time beyond what it normally would be, and which in addition, very likely decrease the accuracy of the judgments.

In the present study therefore, the attempt was made to record judgment-time in such a way, and at such a rate, that the *O* could by no possible cue become aware that his time was being measured. It was proposed, furthermore, to keep the procedure thoroughly in accord with that of the typical psychophysical experiment so that results obtained would be comparable to those of similar investigations where times were not measured.

Procedure. The method of constant stimuli was selected for this purpose, and after the plan of an earlier study, judgments were rendered (a) when an intermediate category of 'equal' judgments were permitted, and (b) when only the two categories of 'plus' and 'minus' were allowed. The stimuli judged and the individuals who judged them were the same throughout both the two-category and three-category series, so that the difference between the methods may be regarded essentially as a difference in the attitudes with which the observers approached their separate tasks.

Observers. The *Os* were 5 graduate students in psychology—3 men (*X*, *Y*, and *W*) and 2 women (*U* and *Z*)—all mature and experienced subjects. They had all served in an earlier investigation of similar nature,[13] and had made more than 4000 psychophysical judgments apiece. Since the apparatus and experimental conditions were identical in the two studies, the training received by the *Os* in the earlier may be regarded as an intensive practice series for this experiment.

The *Os* were kept ignorant of the purpose of the present study by the fact that the data were obtained immediately following the completion of the earlier. There was no break in the continuity of daily sittings and the procedure, as far as the *Os* were concerned, was identical in the two experiments. No obvious means were available, therefore, by which they could know that slight changes in the apparatus upon *E*'s side of the screen enabled the recording of judgment times when none had been taken originally.

Apparatus. Although a detailed description of the apparatus has already been published,[14] a recapitulation of the essential features will be given here for the sake of coherence. The mechanism for producing the visual intensities was a specially constructed Woodworth photometer fitted with a Lummer-Brodhun photometer cube. The source of illumination was a single 21 cp. automobile headlight lamp, the light from which was reflected in two directions through mirrors to produce the pairs of stimuli desired. A timing device for causing the light to flash on and off intermittently was run by a constant speed motor at such a rate that the photometer lamp lighted for a period of 1 sec.

[13] See footnote 1.

[14] W. N. Kellogg, An improved photometer for measuring brightness thresholds in hundredths of a meter candle, *J. Gen. Psychol.*, 2, 1929, 369-77.

(during which *O* was presented with a pair of stimuli for judgment) and was out for 2 secs. while *O* recorded his judgment and *E* made a new setting of the apparatus for the ensuing stimulus pair. A continual check on possible changes in the lamp itself was maintained by means of a voltmeter and ammeter set permanently in the circuit.

The two stimuli appeared as the right and left halves respectively of a circular disk of light (seen through the Lummer-Brodhun telescope) which subtended in its entirety a visual angle of about 6°. The left eye only was used in the observations. Involuntary head movements were minimized by means of a chinrest and eyepiece.

Records of the judgments and of the judgment-times were obtained by means of a special graphic technique.[15] An extension arm upon the light carriage of the photometer traced upon the drum of a high speed horizontal kymograph movements of the carriage which were productive of known changes in stimulus intensity. An electromagnetic signal marker in parallel with the photometer lamp recorded the instant of its illumination and extinction. The *O*s' judgments were not verbal but were rendered by pressing signal keys—one key for each category of judgment. The judgments thus made were also marked automatically upon the kymograph drum by means of signal markers. The record was completed by a Jacquet chronograph indicating fifth-seconds. Times were read, however, to the nearest tenth of a second.[16]

Since the action of the electromagnetic marker in parallel with the photometer lamp is almost instantaneous,[17] while the lamp itself has an appreciable latent period, an obvious error was introduced in the measurement of judgment times from the kymograph records. This error was of course constant for all judgments but nevertheless prevented an accurate determination of absolute judgment-times. A method involving the use of an oscillograph was finally worked out whereby the time required for the lamp to come to full incandescence could be readily determined.[18] The latent period for the bulb employed in the experimental work was found to be 0.18 ± .01 sec. This correction was thereupon subtracted from the obtained judgment times used in the later computations.

The entire apparatus was shielded from *O*'s view by means of a screen through which only the necessary parts, *i.e.* telescope, headrest, and keys, protruded.

Stimuli and method. There were 7 pairs of stimuli arranged in arithmetical progression after the usual scheme employed with the method of constant stimuli. In 3 the left half of the visual field was objectively the darker, in 3 the right half was the darker, and in the remaining pair the fields were objectively equal. The intensity at equality was 21.682 meter candles.

[15] W. N. Kellogg, The graphic method in psychophysics, this JOURNAL, 41, 1929, 456-59.

[16] With the aid of a drawing board and a T-square, parallel lines were ruled on the kymographic tracings and a magnifying glass was employed to facilitate their reading.

[17] Any time error which the marker itself may possess is balanced by similar errors in the markers recording *O*'s judgments.

[18] W. N. Kellogg and R. C. Davis, The measurement of the latent time of electric light bulbs, this JOURNAL, 42, 1930, 300-301.

The stimulus-steps are designated respectively as L_3, L_2, L_1, E, R_1, R_2, R_3. The symbol L_3 indicates that for this pair of stimuli the left screen was darker (less intense) than the right by the greatest amount used, L_1 means the left screen was darker than the right by the smallest difference used, while L_2 is the intermediate step between L_1 and L_3. At 'E' the two screens were objectively equal. Similar settings of the photometer at R_1, R_2, and R_3 produced pairs of stimuli in which the right screen was darker than the left in amounts corresponding to the three 'left screen darker' steps.

The increment of difference between the stimuli compared was not the same for all *Os*, but was adjusted in preliminary observations upon each individual (at the beginning of the series of experiments) so that the percentage of correct judgments of the extreme steps fell between 95 and 100. By this method a group of stimuli of about the same degree of subjective difficulty was employed with each *O*. The large individual differences in the capacity to distinguish intensities were thus equated by the adjustments of the apparatus so that the separate psychometric curves from the judgments of the different *Os* were approximately the same.

When determinations were made according to the two-category series the *Os* were permitted to judge only 'left screen darker', or 'right screen darker.' If the left and right halves of the visual field appeared 'equal,' instructions were to make the best possible guess. In the two-category series, therefore, *O* used only two signal keys, the left-hand key indicating the category 'left screen darker' and the right-hand key indicating the category 'right screen darker.' When judgments were to be grouped into the three-categories of 'left,' 'right,' and 'equal' a third key was added between the other two keys. Suitable orders of rotation precluded possible deleterious transfer effects from one technique to the other.

At the completion of every group of 42 judgments a 1-min. rest period was allowed. In this interval *O* made a retrospective estimate of confidence for the immediately preceding series of 42 judgments. The *O* was asked "of the judgments just completed about what percent were you sure of—in about what percent would you say guessing played no part?" While undoubtedly of less precision than confidence ratings upon each individual judgment, estimates of this sort indicate something nevertheless of the *O*'s subjective impressions throughout a given series of determinations. Estimates following every judgment would have been difficult to record by the graphic method and were not considered of sufficient importance to demand the radical changes in apparatus and procedure which their obtaining would have necessitated.

In order to avoid placing undue emphasis upon one category at the expense of another, the *Os* were directed to regard each of the categories of judgment of the same importance. The following sorting analogy was employed to make this clear. (a) For two-categories: "You may imagine you have two boxes in front of you into which you are sorting the judgments as you make them—in a way similar to that in which you might sort cards." (b) For three-categories: "Imagine you have three boxes, no one of different size or of greater importance than the others. The judgments are then simply to be sorted into the three boxes."

No specific instructions were given regarding the disposition of doubtful as contrasted with genuinely equal judgments. It was assumed, however, that since the same emphasis was placed upon all types of judgments there would be approximately as equable a distribution of doubtfuls among the three possible classes of the three-category plan, as there would be between the categories of right and left when only these two alternatives were allowed. That this assumption was justified in the cases of four of the 5 *Os* is proven by the introspections. The special case of Z will be taken up further in the discussion of results.

Results

The data for the present study were obtained in two consecutive daily sessions for every *O*, during which 672 judgments were made. Computations are therefore based on a total of 3360 timed judgments, half of which are divided into two and the remaining half into three categories.

TABLE I

Showing for Every *O* the Average Judgment-Time for Every Stimulus-Pair (Time in Sec.)

Ob.	No. of categories	Stimuli L_3	L_2	L_1	E	R_1	R_2	R_3
U	two	0.687	0.793	0.895	0.908	0.885	0.772	0.670
	three	0.693	0.860	0.929	0.953	0.975	0.791	0.645
W	two	1.018	1.120	1.314	1.289	1.058	1.008	0.875
	three	1.077	1.179	1.331	1.316	1.222	1.058	0.906
X	two	1.058	1.181	1.145	1.191	1.060	0.989	0.860
	three	1.104	1.406	1.381	1.404	1.266	1.066	1.024
Y	two	0.812	1.003	1.031	1.148	0.989	0.860	0.768
	three	1.004	1.123	1.245	1.206	1.212	1.075	0.860
Z	two	0.889	1.062	1.504	1.518	1.270	0.966	0.793
	three	0.931	1.216	1.706	1.562	1.366	0.956	0.830
Ave.	two	0.893	1.032	1.178	1.211	1.052	0.919	0.793
Ave.	three	0.962	1.157	1.318	1.288	1.208	0.989	0.853

(*a*) *Judgment-times for every pair of stimuli.* Marked individual differences appear in the speed at which judgments are made. Table I shows the extent of these differences. Every figure in this table, with the exception of the two bottom rows, gives the average time (in sec.) of 48 judgments. With the single exception of the times for *U* in the judgment of R_3, all judgment-times are less in the two-category series than in the three. Thus the simple change in attitude which demands that the *O* be ready to judge either 'right' or 'left' in the one case and that he be ready

in the other to judge the same stimulus-pair as either 'right,' 'left,' or 'equal' considerably slows up the average time of judgment.

It appears furthermore that irrespective of the number of categories the judgment-times tend to increase as the difference between the stimuli judged becomes less. Thus the times for the E, L_1, and R_1 are longer than those of the extreme steps.

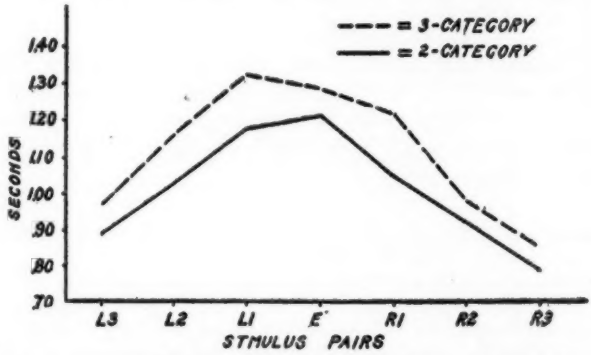

FIG. 1. AVERAGE TIMES FOR 5 *Os* IN JUDGING SEVEN STIMULUS PAIRS ACCORDING TO TWO-CATEGORY AND THREE-CATEGORY TECHNIQUES

The relative magnitudes of these two types of time differences are graphically presented in Fig. 1 where the average times for all *Os* (two bottom rows of Table I) are plotted. An examination of these curves discloses an average increase in the three-category judgment-time of about 10% over the time of the two-category. On the other hand, when the difference between the stimuli is decreased from a point at which the *O* makes approximately 95% correct judgments to a point at which the stimuli are objectively equal (0% correct when two categories are used) the time of judgment increases on the average 40 to 45%.

(*b*) *Judgments grouped according to category.* In Table II the judgment-times have been divided according to the categories in which the *Os* made them without reference to the particular stimulus-pairs which were judged. In the upper half of the table the average times for every *O* of all judgments made under the two-category series are compared with the times of the *equality* judgments made in the three-category series. The average judgment-time of *U* in the two-category series was, for example,

TABLE II

SHOWING FOR EVERY *O* THE TIMES OF RIGHT AND LEFT, AND OF EQUAL JUDGMENTS

		R and L Judgments				Equal Judgments						
	Observer	Ave.	No.	σ	$\sigma_{ave.}$	Ave.	No.	σ	$\sigma_{ave.}$	Diff.	$\sigma_{Diff.}$	D/$\sigma_{Diff.}$
2-Category Series	*U*	0.801	336	.236	.0123	0.920	69	.200	.0241	.119	.0271	4.39
	W	1.097	336	.349	.0191	1.459	46	.318	.0469	.362	.0506	7.15
	X	1.069	336	.397	.0217	1.438	85	.420	.0456	.369	.0505	7.30
	Y	0.944	336	.274	.0149	1.298	91	.289	.0303	.354	.0338	10.48
	Z	1.143	336	.490	.0267	1.739	86	.471	.0507	.596	.0573	10.41
	Ave.		336				75.4					7.95
	Weighted Ave.	1.013				1.381						
3-Category Series	*U*	0.813	267	.256	.0157	0.920	69	.200	.0241	.107	.0288	3.71
	W	1.107	290	.369	.0217	1.459	46	.318	.0469	.352	.0517	6.81
	X	1.167	251	.369	.0233	1.438	85	.420	.0456	.271	.0512	5.29
	Y	1.031	245	.295	.0188	1.298	91	.289	.0303	.267	.0356	7.49
	Z	1.047	250	.404	.0257	1.739	86	.471	.0507	.692	.0568	12.19
	Ave.		260.6				75.4					7.10
	Weighted Ave.	1.033				1.381						

0.801 sec. with a σ of 0.236 sec. and a $\sigma_{ave.}$ of 0.0123.[19] *U* made 69 judgments of 'equal' in the total of 336 judgments in the three-category series, the average time of which was 0.920 sec., with a σ of 0.200 and a $\sigma_{ave.}$ of 0.0241. The difference between the two averages was 0.119 sec. and the $\sigma_{diff.}$, 0.0271. The $D/\sigma_{diff.}$ value, it will be noted, is considerably larger than that demanded for complete statistical reliability (3.00). The same type of relationship exists in the data of the other *Os*, *i.e.* the equality judgments average throughout longer than the R and L judgments, and the difference is statistically significant.

In the lower half of Table II, times for the 'right' and 'left' judgments made in the three-category series are compared with the times of 'equality' judgments. The differences here are also in each case considerably greater than necessary for complete reliability.

(*c*) *Correct and incorrect judgments.* If the judgments are now divided into two groups of correct and incorrect,[20] on the basis of their agreement with the objective relations between the stimuli judged, and if the times for the rights and wrongs thus grouped are compared, the differences shown in Table III are found. It will be noted from this table that it takes longer without exception (on the average) to make a false judgment than it does to make a correct one, regardless of whether two or three

[19]The formulae used in the computations for Table II and for Table III were:

$$\sigma = \left(\frac{\Sigma FD^2}{N} - c^2\right)^{\frac{1}{2}} \left(\text{length of class interval}\right)$$

$$\sigma_{ave.} = \frac{\sigma_{dis.}}{\sqrt{N}}$$

$$\sigma_{diff.} = \left[\left(\sigma_{ave.\ 2\text{-cat.}}\right)^2 + \left(\sigma_{ave.\ 3\text{-cat.}}\right)^2\right]^{\frac{1}{2}}$$

[20]For the L stimulus-pairs, a judgment of 'left screen darker' would of course be correct, while judgments of 'right screen darker' and 'equal' would be incorrect. Similarly for the R stimulus-pairs 'right screen darker' judgments are correct and judgments in the other two categories are incorrect.

'Equal' judgments in the three-category series are correct only for the middle stimulus-pair of the seven (in which the two stimuli were objectively the same). A division into correct and incorrect classes of those judgments of the E stimulus-pair made by the two-category method is not possible since the instructions themselves prevent the making of any correct judgments. Judgment times for the E step, two-category, are therefore omitted from the figures in Tables III and IV.

TABLE III

SHOWING FOR EVERY *O* THE TIMES OF CORRECT AND INCORRECT JUDGMENTS

		Correct				Incorrect						
	Observer	Ave.	No.	σ	$\sigma_{ave.}$	Ave.	No.	σ	$\sigma_{ave.}$	Diff.	$\sigma_{Diff.}$	$D/\sigma_{Diff.}$
2-Category Series	*U*	.771	264	.229	.0141	.920	24	.187	.0372	.149	.0397	3.75
	W	1.022	244	.314	.0201	1.295	44	.321	.0484	.273	.0523	5.22
	X	1.018	234	.381	.0249	1.183	54	.422	.0574	.165	.0625	2.64
	Y	.880	254	.230	.0144	1.138	34	.292	.0501	.258	.0522	4.94
	Z	1.032	269	.423	.0258	1.773	19	.400	.0918	.741	.0954	7.77
	Ave.		253				35					4.86
	Weighted Average	.943				1.224						
3-Category Series	*U*	.785	255	.237	.0149	.993	81	.198	.0220	.208	.0266	7.82
	W	1.085	246	.354	.0226	1.346	90	.386	.0407	.261	.0466	5.60
	X	1.152	241	.377	.0243	1.446	95	.438	.0449	.294	.0511	5.75
	Y	1.024	233	.276	.0181	1.282	103	.317	.0312	.258	.0361	7.14
	Z	1.088	272	.427	.0259	1.795	64	.487	.0609	.707	.0662	10.68
	Ave.		249.4				86.6					7.40
	Weighted Average	1.066				1.352						

categories of judgments are employed. The differences between corrects and incorrects are all statistically reliable except in the case of *X* (two-category), and even for this individual the obtained difference is great enough to be significant 996 times in 1000.

Henmon's findings that incorrect discriminative reaction-times tend to be more variable than correct ones do not seem to apply under the conditions of the present experiment where the *O*s made judgments without the impetus of excessive speed.[21] Although a number of the absolute dispersion measures of the judgment-times of the separate *O*s are larger when incorrect judgments are made, the *relative* variabilities (figures not presented here) are found to be smaller for incorrect than for correct judgments in all instances but one, throughout both two-category and three-category data.[22]

Three other important facts are indicated from the figures in Table III. (1) The difference between times of correct and incorrect judgments is greater on the average with three categories than with two. A possible explanation for this lies in the fact that a considerable number of the incorrect determinations by the three-category technique are judgments of 'equal' which have been shown in Table II to require a longer time than the judgments of the other classes. (2) The times for both correct and incorrect judgments are in general slower in the three-category series than in the two, as indicated in the weighted averages of the times of all *O*s. We have here further evidence (after Merkell) that the attitude of preparation for three alternatives holds up the judgment longer than the preparation for two—and this quite independently of the correctness or incorrectness of specific judgments. (3) About 12% (35 out of 288) of all the judgments in the two-category series are incorrect, whereas about 25% (86.6 out of 336) in the three-category series are incorrect. These percentages offer striking corroboration of the operation of the 'laws of chance' in psychometric determinations. With three possible classes of judgment, only one of which can be correct, the *O* has exactly twice the opportunity of judging incorrectly that he has with two classes. The obtained figures are therefore remarkably close to the theoretical values expected.

A different grouping of correct and incorrect judgment-times is given in Table IV. The times of all *O*s are pooled in this table

[21] V. A. C. Henmon, *op. cit.*, *Psychol. Rev.*, 18, 1911, 195.

[22] By means of Pearson's coefficient of variation in which the coefficient $V = 100\,\sigma/\text{ave}$.

and the correct judgments are separated from the incorrect *by stimuli*. With few exceptions every wrong time is longer than its corresponding correct time and every time (either correct or incorrect) in the two-category series is faster than its corresponding time in the three-category series.

TABLE IV

SHOWING THE AVERAGE TIMES OF CORRECT AND INCORRECT JUDGMENTS FOR EVERY STIMULUS-PAIR

(Data for all *O*s pooled)

	Judgment	L_3	L_2	L_1	E	R_1	R_2	R_3	Weighted Ave.	Total No.
2-Category Series	Correct	.884	1.012	1.151		1.001	.904	.776	.943	
	No.	228	217	167		195	224	234		1265
	Incorrect	1.045	1.316	1.237		1.278	1.120	1.437	1.224	
	No.	12	23	73		45	16	6		175
3-Category Series	Correct	.928	1.062	1.843	1.320	1.118	.954	.848	1.066	
	No.	230	181	82	142	158	216	238		1247
	Incorrect	1.730	1.428	1.355	1.242	1.381	1.308	1.420	1.352	
	No.	10	59	158	98	82	24	2		433

A definite negative relationship is suggested between the number of correct judgments and the average time for any given step, *i.e.* the less the time the greater the number of judgments. The same sort of a connection (although much less marked—possibly because of the smaller number of cases) appears between the wrong times for every step and their number, *viz.* the fewer the incorrect judgments the slower the time of judgment tends to be.

(*d*) *Psychometric time-curves.* An analysis of the different classes of judgment made on each stimulus-pair is presented in Table V. The data of all *O*s are here treated together as in the preceding table. In addition to columns of "Ave. Time" and "No. of Judgments" a column of "Relative Frequencies" is included from which relative frequency psychometric curves of the usual type may be plotted.[23] The time and frequency data given in this table are shown graphically in Figs. 2 and 3. The relative

[23]Cf. F. M. Urban, The application of statistical methods to the problems of psychophysics, 1908, 111-166.

frequency curves for the different categories are not drawn on the same coördinates (according to the customary method), but each is treated in a separate graph on which also are the time-curves for the corresponding judgments. In these charts the stimuli are

TABLE V

SHOWING THE AVERAGE TIMES OF DIFFERENT CLASSES OF JUDGMENT FOR EVERY STIMULUS-PAIR

(Data for all *O*s pooled)

2-Category Series					3-Category Series				
Stimuli	Judgment	Ave. Time (Weighted)	No. of Judgments	Relative Frequency	Stimuli	Judgment	Av. Time (Weighted)	No. of Judgments	Relative Frequency
L_3	L	0.884	228	.950	L_3	L	0.928	230	.958
	R	1.045	12	.050		R	1.653	6	.025
	E					E	1.845	4	.017
L_2	L	1.012	217	.904	L_2	L	1.062	181	.754
	R	1.216	23	.096		R	1.292	25	.104
	E					E	1.558	34	.142
L_1	L	1.151	167	.696	L_1	L	1.245	82	.341
	R	1.238	73	.304		R	1.325	41	.171
	E					E	1.366	117	.488
E	L	1.189	95	.396	E	L	1.258	32	.133
	R	1.224	145	.604		R	1.234	66	.275
	E					E	1.320	142	.592
R_1	L	1.278	45	.187	R_1	L	1.345	16	.067
	R	1.000	195	.813		R	1.118	158	.658
	E					E	1.389	66	.275
R_2	L	1.120	16	.067	R_2	L	1.203	12	.050
	R	0.904	224	.933		R	0.954	216	.900
	E					E	1.412	12	.050
R_3	L	1.437	6	.025	R_3	L	1.020	1	.004
	R	0.776	234	.975		R	0.848	238	.992
	E					E	1.820	1	.004

indicated along the horizontal coördinates, the scales for judgment-times are on the left vertical coördinates and the relative frequency scales on the right vertical coördinates. Relative frequency curves are made by the heavy dotted lines, while the heavy solid lines trace the judgment-time curves (data for all *O*s averaged). The light solid lines show the individual time-curves for each *O* respectively, the figures for which are omitted from the published tables. The curves are presented, however, to show

the scatter of the individual times about the average (the heavy solid) line and to demonstrate the tendency of all curves despite individual differences to trace the same general kind of pathway.[24]

The time-curves, to be sure, fluctuate considerably when compared with the smoother frequency curves. It should be noted, however, that this irregularity is less for those portions of the R

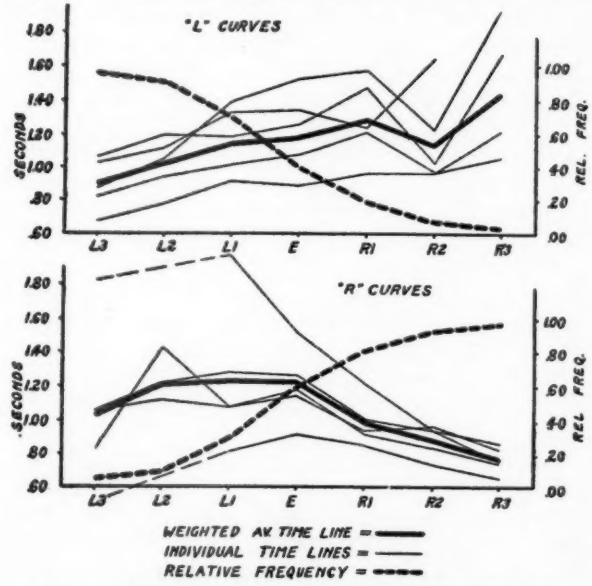

FIG. 2. PSYCHOMETRIC TIME-CURVES DERIVED FROM THE TWO-CATEGORY SERIES.

and L curves which represent the correct judgments (the L_1, L_2, and L_3 points for the L curves and the R_1, R_2, and R_3 points for the R curves). This suggests that the increase in fluctuations among the wrong judgments is traceable to the smaller number of wrong cases since the incorrects as a group have been shown in Table III to be but a fraction of the number of correct judgments.

[24]The occasional ending of the fine solid lines short of the extreme stimulus-steps indicates that the *O* made no judgments of the category plotted for the stimuli in question. The discontinuous prolongations of certain of the individual time curves indicate similarly the omission of judgments for the intermediate stimuli but the presence of judgments at the two extremes of the discontinuous section.

By arranging the relative frequency curves (heavy dotted lines) and the corresponding averaged time-curves on the same charts, a ready comparison can be made between them. When the

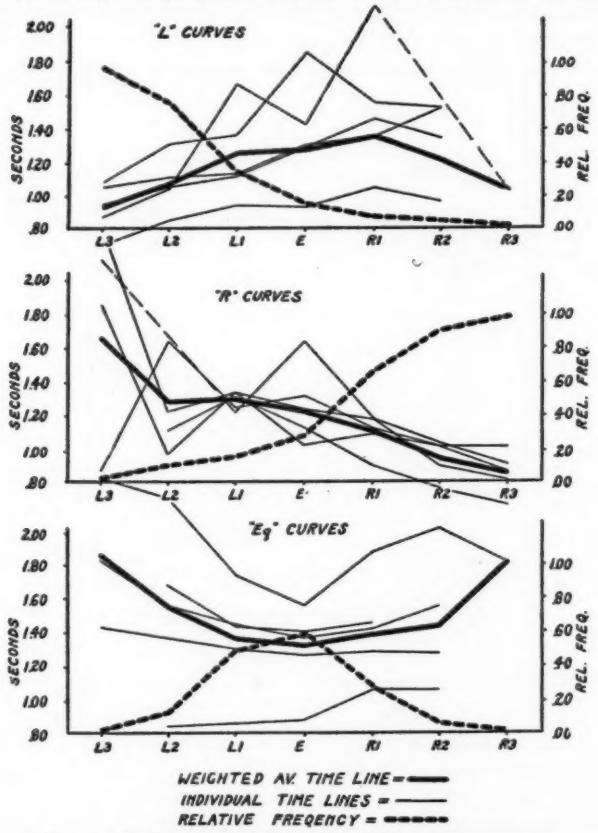

FIG. 3. PSYCHOMETRIC TIME-CURVES DERIVED FROM THE THREE-CATEGORY SERIES.

frequency curves are high, the corresponding time-curves, it will be seen, are at their lowest points. Similarly, when the frequency curves drop, the time-curves tend to rise (excepting minor fluctuations). This association applies in general for curves of the R, L, and E classes in both the two- and the three-category series. The

conclusion seems warranted therefore that the *psychometric time-curves for all categories tend to be inversions of the psychometric relative frequency curves.*

If one considers next the relationship of the time-curves to the stimuli it appears for the R and L curves (of both Fig. 2 and Fig. 3) that the smaller the difference between the stimuli the greater the time becomes *throughout the sections of the curves which represent correct judgments.* Up to this point the results from judgment-times corroborate in general Henmon's finding on discriminative reaction-time.[25] It is possible, however, from the psychometric time-curves to carry the connection one step farther in the statement that *when the difference between the stimuli reduces to zero and then assumes a negative (reversed) value, the time of judgment tends to continue its increase.*

This common result may be independently applied to each of the R and L categories of judgment, somewhat as follows: (a) 'Left screen darker' judgments of stimulus-pairs the left member of which becomes progressively less dark, require a progressively longer time. (b) 'Right screen darker' judgments of stimuli the right one of which becomes progressively less dark, require progressively longer times. But if at this point the time curve for judgments of the 'equal' class (lower chart, Fig. 3) is examined it appears further that (c) the *less equal* the stimuli are the longer the average time required to make an equal judgment, or put in another way, as the stimulus-pairs deviate farther and farther from objective equality, equal judgments made of those stimuli tend to consume a longer time.

We may finally consolidate the three relationships for the separate categories in a single general principle, *viz., within any given category of judgment, the more the stimuli deviate from the ideally perfect condition typified by the category name, the longer the judgment-times tend to become.*

The association between stimulus-differences and judgment-times proves beyond a doubt that the greater average time consumed in making equality judgments (brought out in Table II) cannot be ascribed to the fact that these judgments are more likely to occur when the difference between the stimuli is small. For regardless of the frequency of occurrence of the equal judg-

[25] V. A. C. Henmon, *op. cit., Arch. Philos., Psychol. & Sci. Methods,* 1906, (no. 8).

ments their time is greatest when the difference between the stimuli is large. What then causes the equal judgments as a group to require a longer time than those of the other categories? Is it because many of the judgments thrown into the equal class are accompanied by a large element of doubt? George,[26] who separated doubtful from equal judgments by a careful introspective technique, found that *bona fide* equal judgments, quite free from any element of doubt, were still slower than other classes of judgment. Similar evidence is offered from the retrospections of *U*, *W*, *X*, and *Y* which indicate that there were no more doubtful cases among their equal judgments than there were among their R and L judgments. Any hesitancy resulting from doubt must therefore have been about as effective throughout one category of judgments as it was for the others. As a result of these considerations, we seem forced to the conclusion that there is some further factor, as yet not clearly perceived, which makes the equal judgment more difficult and hence slower to render. (One might, after George, call it the 'reflective' element although little is gained in the arbitrary assignment of a name). At present this factor can be regarded as an intrinsic characteristic of the equality judgment itself. It is certainly not to be accounted for either on the basis of stimulus-differences, or on the basis of doubt.

Additional light is thrown upon its influence if we consider the average three-category judgment-times of the equal pair of stimuli without reference for the moment to the judgment-times of the other stimuli. According to the three-category time figures in Table V, it requires an average interval of 1.258 sec. to make L judgments, 1.234 sec. to make R judgments and 1.320 sec. to make equality judgments, when objectively equal stimuli are judged. Such a finding is indeed contrary to expectation since equal judgments of equal stimuli are correct whereas R and L judgments of equal stimuli are incorrect. The delaying characteristic of the equality judgment shows itself therefore to be of sufficient potency to function in the face of the opposing right-wrong relationship pointed out in Tables III and IV.

(*e*) *Confidence.* In Table VI, an attempt has been made to ascertain the relationship between times and confidence estimates by presenting the average confidence ratings for each of the successive experimental sittings, with the average judgment-times

[26] S. S. George, *op. cit.*, this JOURNAL, 28, 1917, 1-37.

for the same period. The confidence ratings are in the forms of the percentage of judgments of which each *O* was certain. A higher figure therefore indicates a greater degree of assurance.

TABLE VI

SHOWING FOR EVERY *O* THE CONFIDENCE ESTIMATES AND JUDGMENT-TIMES

	Day		Observers				
			U	*W*	*X*	*Y*	*Z*
2-Category Series	1st	Conf.	75.0	78.8	67.5	30.5	67.5
		Judg.	0.691	1.156	1.055	0.909	1.196
	2nd	Conf.	76.3	81.3	72.5	41.3	76.3
		Judg.	0.911	1.038	1.082	0.979	1.090
	Ave.	Conf.	75.7	80.1	70.0	35.9	71.9
		Judg.	0.801	1.097	1.069	0.944	1.143
3-Category Series	1st	Conf.	80.0	82.5	72.5	37.5	60.0
		Judg.	0.793	1.219	1.290	1.068	1.281
	2nd	Conf.	76.3	83.8	73.8	27.5	63.8
		Judg.	0.877	1.092	1.181	1.139	1.166
	Ave.	Conf.	78.2	83.2	73.2	32.5	61.9
		Judg.	0.835	1.155	1.236	1.103	1.224

From the lower half of Table VI, it will be noted that without exception the confidence for every *O* averages less for those days on which the judgment-times are longer. An inverse relationship of a crude type exists therefore between confidence and judgment-time when the three-category data are compared in this manner. That this condition does not obtain for the two-category data is apparent when the upper half of the table is considered. As a matter of fact no uniform or general association of any type is disclosed in the two-category figures.

The evidence thus presented, though somewhat meager, indicates that judgment-time is a less valid index of confidence in the two-category series than it is in the three-category. As an explanation of this fact it is suggested that the forcing of the *O* to classify judgments into two arbitrary divisions, instead of permitting him to judge equal appearing pairs as 'equal,' may introduce a disturbing or distracting element which prevents *O* from giving as accurate an estimate of his performance as he gives when allowed the added latitude of three judgment-classes.

(*f*) *Attitude and the equal category.* Retrospective reports obtained from the *O*s demonstrate well the difficulty of keeping the criterion of 'equality' the same from one individual to the next. *U*, *W*, *X*, and *Y*, following more exactly the letter of the instructions, all distributed the doubtful cases throughout the different categories. This shows up well in their time data where the slower times, for the same category, occur when three alternatives are allowed. Doubtful judgments in these instances constituted no irregular factor—the longer average times in the three-category series being attributable to the necessity of deciding between three as against two possibilities. For *Z*, however, equal judgments were very difficult to make and were as a group unpleasant.

TABLE VII

SHOWING FOR EVERY *O* AND FOR EACH SERIES THE AVERAGE TIME OF ALL JUDGMENTS

	Observers					
	U	*W*	*X*	*Y*	*Z*	Ave.
Two-Category	0.801	1.097	1.069	0.944	1.143	1.011
Three-Category	0.835	1.156	1.236	1.103	1.224	1.112
R & L Judg. (3-Category)	0.813	1.107	1.167	1.031	1.047	1.033

This *O*, when questioned after the completion of the experiment, was found to have put practically all of her doubtful three-category judgments into the equal class. As a result the R and L judgments made by *Z* according to the three-category technique contained on the whole less doubtful cases than R and L judgments made according to the two-category plan.

The effect of these two opposing attitudes is brought out clearly in Table VII. For all *O*s except *Z* the average two-category time is less than the average time for the R and L three-category judgments. Since the doubtful judgments are largely eliminated from *Z*'s three-category R and L judgments, however, it takes this *O* less time to make R and L judgments in the three-category series than it does to make the same type of judgments in the two-category series. The removal of doubt seems consequently to be more effective in the reduction of judgment-time than the increase in the number of categories is in lengthening it.

SUMMARY

At the completion of an intensive practice series (in connection with an earlier experiment) 5 *O*s rendered a total of 3360 judgments by the method of constant stimuli that were individually

timed. The stimuli judged were 7 fixed pairs of visual intensities produced by a special photometer. Recording of times and of judgments was accomplished automatically by the graphic method. The experiment was so conducted that none of the *O*s had any knowledge that his judgment-times were being recorded. Half of the judgments made by every *O* were divided into the two categories of 'right screen darker' (R) and 'left screen darker' (L), and the remaining half were classified by the observers according to the three-category scheme of R, L, and equal. Following are the principal findings.

(1) The judgment-times in the three-category series average throughout about 10% larger than those in the two-category series. The increase in time is explained as a function of the greater subjective difficulty of being prepared for three possible alternatives in the one case as against the relatively simpler preparation for two alternatives in the other.

(2) When the difference between the stimuli is decreased from a point at which *O* judges 95-100% correctly, to a point where the stimuli are objectively the same (0% correct with two categories) the time of judgment on the average increases 40-45%.

(3) Judgments of equality are uniformly longer than those of the other categories. The time differences between equal and non-equal classes are statistically reliable for all *O*s.

(4) It takes longer to make an incorrect judgment than to make a correct one. Differences here are also for the most part statistically reliable.

(5) About twice as many false judgments are made by the same *O* when he judges according to three categories as when he judges with only two, since in the former case he has twice as many chances of being wrong as he does in the latter. The experimental results strikingly corroborate the theoretical expectations.

(6) The higher the percentage of *correct* judgments in the case of any given pair of stimuli the shorter the average judgment-time for that pair of stimuli tends to be.

(7) Time-curves plotted in a manner analogous to that followed in the customary relative frequency psychometric curves are of the general form of, though more irregular than, the frequency curves, *but they tend to be inversions of these curves.*

(8) It therefore takes longer to make a judgment of equality when the stimuli judged differ markedly from equality, than when they are nearly (or are exactly) equal.

(9) The facts demonstrated in the time-curves of all three judgement categories lead to the general principle that *within any given category of judgment the more the stimuli deviate from the ideally perfect condition typified in the category name, the longer the judgment-times tend to become.*

(10) There is some peculiar characteristic of the equality judgment itself which renders it a slower type of judgment to make than the non-equal judgment even when no element of doubt is present.

(11) The time of judgment is a better index of the confidence of the *O*s when their judgments are permitted to vary among three categories than when they are restricted to two. A suggested explanation of this fact is that the preclusion of the category 'equal' confuses or distracts the *O* when pairs of stimuli are presented which appear actually the same, and so prevents him from making as accurate an estimate of his performance as he makes when permitted the greater latitude of three judgment classes.

THE RELATION OF BRIGHT AND DULL PRESSURE TO AFFECTIVITY

By WILLIAM A. HUNT, Harvard University

In 1924, J. P. Nafe published an account of some experimental work identifying affection with bright and dull pressure.[1] He was dissatisfied with the previous variability of introspective reports of the affective states. To Nafe, the trouble seemed to lie in the use of stimuli which were too strong and which forced the observers to become "feelers" rather than observers of feeling. In his work he used moderate stimuli which would arouse an affective reaction without being too distracting. These stimuli were chosen from several modalities. The observer was told to wait until the affective state had reached a supposed maximum and then to break off his observation and give an introspective report. As the experiment progressed, uniformity gradually appeared in the descriptions. Pleasantness was described as a bright pressure and unpleasantness as a dull pressure; and these pressures were related to those of the same name in Titchener's touch pyramid. Nafe concluded that pleasantness is a "bright pressure" and unpleasantness is a "dull pressure."

P. T. Young attempted to check Nafe's findings.[2] With one of Nafe's *O*s, he got reports of pressures, but his other *O*s failed to duplicate Nafe's results. He denied Nafe's identification of P and U with bright and dull pressure, and concluded that Nafe's results were largely a product of the general laboratory atmosphere then prevailing at Cornell. Nafe's reply was that bright and dull pressures exist, but are not "attention demanding."[3] Thus they would not be reported unless the *O* is set for them. Not only did this idea explain Young's negative results, but also it offered a possible explanation for any negative results which might be

*Accepted for publication June 8, 1930.

[1] J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 507-544.

[2] P. T. Young, Studies in affective psychology, *ibid.*, 38, 1927, 175-185.

[3] Nafe, The psychology of felt experience, *ibid.*, 39, 1927, 367-389.

secured by others in repeating Nafe's experiment. It became increasingly clear that, in order to make a valid check of Nafe's findings, it would be necessary to resort to some form of experimentation other than that used by either Nafe or Young.

The present investigation was undertaken on the assumption that, if a valid and conclusive check is to be made, there are two requirements which have to be fulfilled.[4] In the first place, to satisfy Nafe, the *O*s must be instructed to observe bright and dull pressures. In the second place, to prevent the operation of laboratory bias and thus to satisfy Young, the relation between affection and pressure has to be measured without resort to the *O*'s opinion concerning this matter. Consequently the writer determined to use the method of correlation. To fulfill Nafe's requirement that the *O*s be set for bright and dull pressure, he decided to present them with a description of bright and dull pressure; but to fulfill Young's requirement, he made no mention of any connection between these pressures and affective states. Armed with Nafe's description of these pressures, the *O*s were merely to report on their presence or absence in a series of stimulus-situations. At a later date, with no intimation of any relation between the two sections of the experiment, the *O*s were to be subjected to the same series of stimuli and told to report on the affective states aroused. The correlation between the two sets of reports would then support Nafe's claims or disprove them.

Procedure and Results

A possible criticism of Nafe's work arose from the fact that the greater number of reports of pressure occurred in the case of such modalities as taste, smell, touch, and audition, where the stimulation may produce actual local experiences of pressure. Indeed, preliminary experimentation by the writer showed a tendency to confusion in judgments of bright and dull pressures where local tactual pressures arose from the stimulation of the above modalities. For this reason he selected colors as stimuli.

Fifteen standard Milton Bradley colored papers of various hues and brightness were used. These were exposed through an aperture, 5 in. square, in a large screen, 6 ft. high by 4 ft. wide, with a 2-ft. wing on each side. The screen was painted a cream gray. Its size enabled *E* to sit at a table behind it unseen by *O*. The large gray field aided *O* in concentration by shutting out distracting stimuli. *O* sat in an easy chair, his eyes about 4 ft. away from, and on a level with, the aperture. The position of both chair and screen was kept constant. Lighting was artificial and constant.

[4]The writer wishes to express his indebtedness to Dr. J. G. Beebe-Center of Harvard University for his constant assistance and encouragement.

The *O* was told to sit in a comfortable, relaxed position, and was then presented with the following instructions, in which the descriptions of bright and dull pressure are drawn from various parts of Nafe's published work.

"I shall present various colored stimuli. You are to attend to the general experience involved in observing them, and to state whether this experience involves a 'bright pressure' or a 'dull pressure.' These pressures are qualitatively similar to those ordinarily elicited by cutaneous stimulation, but are in no way identical with them.

" 'Bright pressure' has been described as tactual bright pressure or a touch quality lying between bright pressure and tickle. It has expansive qualities and has been called radial, being more prominent at the edges than in the center. It is bright, buoyant, and has a diffuse bodily feel. The whole experience is diffuse and volumic, and is patterned, with the patterns varying from one experience to another. The following adjectives have been used in describing it: bright, sparkling, brilliant, active, effervescent, like tickle, dancing, shimmering, misty, mild, vaporous, light, airy, fluffy, ethereal, smooth, soft, welling, spreading, like expansiveness of the body.

" 'Dull pressure' has been described as dull pressure or a quality lying near dull pressure, or between dull pressure and strain. It is not extended. It has qualities of constriction, and has been called radial, but more prominent at the center than at the edges. While it is diffuse and volumic, it is less so than bright pressure and is duller and less bright. It has been called dull, massive, drab, dead, rigid, less lively, somber, inert, stiff, gloomy, more dense, heavy, sinking, leaden, thick, cold, rough, grating, insistent, condensed, like bodily contraction."

The series of colors was then presented three times, in constant order, with about a 10-sec. interval between successive colors. There was a brief rest at the end of each series. Judgments were recorded and *O* was dismissed for one week.

The next week he was given the following instructions: "I shall present various colored stimuli, and you are to judge whether they are pleasant, unpleasant, or indifferent." The same colors and procedure were used. A week later the pressure judgments were repeated, and the following week a final series of affective judgments was called for. Thus the experiment covered four consecutive weeks. Six *Os* were used; three of them, Mr. H. Kim (*K*), Mr. C. Goldthwait (*G*), and Miss F. Cutujian (*C*), graduate students in psychology at Harvard and experienced *Os*, and the other three, Mrs. E. Hunt (*H*) Miss D. Bossen (*B*), and Mr. W. James (*J*), totally inexperienced in observing (the last two had never done any work in psychology).

In the treatment of results the pressure judgments were given numerical symbols; a judgment of bright pressure was assigned a value of +1, indifference 0, and dull pressure −1. These symbols were then added for each color, and their sum was considered to represent the degree to which the color elicited bright pressure rather than dull pressure. The same procedure was employed for the affective judgments; a value of +1 was assigned to pleasantness, 0 to indifference, and −1 to unpleasantness, and the sum of these values for any color was considered to represent its pleasantness. The degree to which colors elicited bright pressure was then correlated with their pleasantness by the product-moment formula. The results were as follows:

Observer	r	P.E.
K	0.72	0.08
G	0.94	0.02
J	0.86	0.05
H	0.88	0.04
B	0.96	0.03
C	0.99	0.003

Because this experiment might have offered *O* some clue as to the nature of the experiment after the second pressure judgment, a check experiment was performed in which a single series of pressure judgments was followed, after one month, by a single series of affective judgments. Five more colors were added, bringing the total to twenty, and each color was presented five times at each sitting. Three wholly naive observers were used, none of whom had done any previous observing or any psychological study. The results confirmed the previous work.

Observer	r	P.E.
L	0.82	0.05
L_2	0.92	0.02
T	0.88	0.03

In order to ascertain whether the preceding pressure series had possibly set the *O*s for internal processes in the subsequent affective series, two new wholly naive *O*s were given the two series as above with the affective judgments called for *first*, and the pressure judgments obtained on the following day. The results follow:

Observer	r	P.E.
H_2	0.92	0.02
H_3	0.94	0.02

These correlations are exceedingly high, particularly in view of the damaging influence of affective variability.

In order to determine whether the correlations would approximate +1.00 when corrected for this variability, the attenuation formula proposed by Spearman was applied to the data yielded by the first part of the experiment (the part using 6 *O*s and covering four consecutive weeks). The Spearman formula is:

$$r_{uv} = (r_{u_1v_1} \cdot r_{u_1v_2} \cdot r_{u_2v_1} \cdot r_{u_2v_2})^{\frac{1}{4}} / (r_{u_1u_2} \cdot r_{v_1v_2})^{\frac{1}{2}}$$

Letting u_1 stand for the first pressure series, u_2 for the second pressure series, v_1 for the first affective series, and v_2 for the second affective series, application of the attenuation formula yields the following results for the 6 *O*s.

Observer	Original r	Corrected r
K	0.72	0.84
G	0.94	1.02
J	0.86	0.99
H	0.88	1.02
B	0.96	0.96
C	0.99	1.01

Thus the application of the attenuation formula gives a series of corrected r's which indicate that, if the effect of variability upon our correlations could be cancelled out, the correlations would approach a value of +1.00, or perfection.

Although localization was not mentioned in the instructions, two *Os* spontaneously localized dull pressure in the region of the stomach or abdomen, and bright pressure in the upper part of the chest. Subsequent questioning of the other *Os* confirmed this localization, although one lumped the pressures all together in a region just over the stomach. All the *Os* said the pressures were simple experiential elements.

As a final check, it was decided to establish the pressure rank order of a series of colors for one group of *Os* and then to compare this with the affective rank order of the same colors determined for a different group of *Os*. Due to the influence of the sampling error, it was expected that the correlation would not be as high as those previously obtained. A positive result, however, would be a further link in the chain of evidence.

Apparatus and procedure were the same as before. Fifteen colors were used, with three presentations each. The descriptions of bright and dull pressure were simplified, and a report of "nothing" was substituted for "indifferent" in the instructions, in case the latter word had been furnishing a cue. The revised pressure instructions follow:

"I shall present various colored stimuli. You are to attend to the general experience involved in observing them, and to state whether this experience involves a 'bright pressure' or a 'dull pressure'. Should neither be present, give a report of 'nothing'.

" 'Bright pressure' resembles tactual bright pressure, or a touch quality lying between bright pressure and tickle. It is a diffuse bodily feeling with expansive qualities. In addition to its decided brightness, it may be described as buoyant, lively, elusive, and welling. It is like expansiveness of the body.

" 'Dull pressure' has been described as dull pressure or a quality lying near dull pressure, or between dull pressure and strain. It is not extended, and has qualities of constriction. It is dull, inert, heavy, sinking, leaden, condensed, and like bodily constriction."

Sixteen new *Os*, members of an introductory course in experimental psychology, were used in determining the pressure rank order. Five of these had to be discarded, one because of color blindness, and four because of their inability to assume the correct introspective attitude. These four had made their judgments on the basis of color brightness rather than pressure. Eleven other new *Os*, members of the Laboratory, were used to establish the affective rank order of the colors, which was then compared with the pressure rank order.

Assigning purely arbitrary letters to the individual colors, the average rank orders for the eleven observers were as follows:

Pressure	*n, c, o, a, k, j, l, d, b e g, m, f, h, i*
Affection	*n, o, k e, g, a, j, c, d, m, l, b, h, f, i*

Correlation of these two series by the rank-order formula gives a coefficient of 0.75, with a probable error of 0.08.

Among the eleven series of pressure judgments, there were five cases of spontaneous reports of localization. Dull pressure was reported as in the abdomen or low in the chest, while bright pressure was always localized above it in the upper regions of the chest.

Conclusions

These results obviously lend support to Nafe's thesis. They do not, however, prove it. It seems to the writer that three different interpretations are possible.

(1) Affection *is* bright and dull pressure. This is Nafe's view.

(2) Affection is accompanied by bright and dull pressure. This is the view held by the writer.

(3) Affection and bright and dull pressure depend upon identical processes in proprioceptive end-organs, but upon different attitudes of report.

Which of these views is correct can only be decided by further investigation.

The reports also seem to confirm Horiguchi's[5] localization of bright pressure in the upper regions of the chest and dull pressure in the region of the abdomen, although the writer prefers to reserve final judgment on this point until the conclusion of work now in progress.

[5]L. B. Hoisington, Pleasantness and unpleasantness as modes of bodily experience, *Wittenberg Symposium on the Feelings and Emotions*, 1928, 237-240.

A STUDY OF VOCATIONAL ATTITUDES IN RELATION TO PUBESCENCE

By HARVEY C. LEHMAN, Ohio University, and
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Students of child growth have been attempting for many years to determine and to express with quantitative exactness the relationship between the child's stage of physiological development and his status in several behavior manifestations.[1] Since both types of measurement present difficulties, statements of precise relationship are rarely found. If physiological growth proceeded at an identical or even approximately equal rate for all children, the problem would be somewhat simplified. Enormous individual differences in rate of growth present important obstacles to exactness in quantitative measurement and expression of physiological maturity. Illustrative of the wide individual differences in physiological growth are data assembled by Engström regarding age of first menstruation.[2] Of 3,500 Finnish subjects studied by Engström, 2 experienced their first menstruation at the age of 8 years, and 1 not until age 26. This range (18 years) shows clearly the individual differences in rate of physiological development; and this measurement expresses of course only one of the numerous elements involved in physiological maturity.

In view of such wide differences in physiological growth and development, it seems plausible that differences in physiological maturation may be related positively to differences in the maturation of certain behavior manifestations. In a previous article, one of the writers presented data showing a positive relationship between the onset of pubescence in school girls and the decline of their interest in doll play.[3] Using data assembled by the Baltimore Public Athletic League, the writer constructed a curve showing the age of first menstruation for certain American girls and a second curve showing the percentages of girls of various age levels who indicated that they had participated in doll play during the course of one week. The curves exhibited startling similarity.

Furfey commented that it is particularly essential to know, not only the percentages of children of various ages who are interested in a certain activity, but also *the percentage of participating prepubescents* who continue to participate in the activity *after* the onset of pubescence.[4] Furfey proceeded therefore to determine from his own data the number of 12-yr.-old boys who were interested

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[1]This study presents some of the findings that have been made possible by a grant in aid from the Social Science Research Council. The writers are also indebted to Mr. A. J. Stout and Mr. George A. Melcher for assistance in securing the data.

[2]Quoted by L. S. Hollingworth, *The Psychology of the Adolescent*, 1928, xiii, 259 (3).

[3]H. C. Lehman, A study of doll play in relation to the onset of pubescence, *Ped. Sem.*, 34, 1927, 72-76.

[4]P. A. Furfey, Pubescence and play behavior, this JOURNAL, 41, 1929, 109-111.

in several play activities. He then computed the percentages of these children who retained interest in the several play activities at successive age-levels. In making his computations Furfey assumed that the present 12-yr.-olds will respond one year hence as the 13-yr.-olds now respond, that the present 12-yr.-olds will respond two years hence as the 14-yr.-olds now respond, etc. This assumption is necessary unless a special group is studied genetically.

The writers of this paper feel, like Furfey, that attention should be directed to the percentages of children who *retain* an activity interest after pubescence.

TABLE I

TOTAL NUMBER OF WHITE BOYS AND GIRLS INCLUDED IN 5 STUDIES OF VOCATIONAL ATTITUDES

	Age-Groups											Totals
	8½	9½	10½	11½	12½	13½	14½	15½	16½	17½	18½	
	203	349	403	387	392	365	361	324	251	162	57	3,254
Boys	82	257	347	385	429	363	352	285	239	160	69	2,968
	137	212	261	259	331	373	291	294	248	132	48	2,586
	304	342	430	356	313	252	246	209	172	106	43	2,773
	140	182	236	253	269	235	166	132	93	46	13	1,765
Totals	866	1342	1677	1640	1734	1588	1416	1244	1003	606	230	13,346
	259	358	372	375	372	346	391	322	299	156	36	3,286
	101	274	340	355	337	378	329	343	261	171	44	2,933
Girls	180	211	238	272	326	317	345	305	217	111	33	2,555
	324	359	435	377	332	298	282	241	197	95	19	2,959
	174	203	222	228	266	222	164	181	95	37	7	1,799
Totals	1038	1405	1607	1607	1633	1561	1511	1392	1069	570	139	13,532
									Grand total			26,878

The reason for this direction of attention follows: the onset of pubescence takes place in every normal child and individual differences are marked. Nevertheless, individual differences in play interests are so great that *no one* play activity is participated in by *all* children during the periods studied by the writers. Because of this, if two curves are drawn, one representing the onset of pubescence and the other the waning of a given play interest, it is clear that these curves will not be strictly comparable since the play curve will not start at the 100% level as does the curve for the onset of pubescence. Therefore the relationship existing between the waning of interest in a particular activity and the onset of pubescence is always partially obscured by individual differences in activity participation unless the number of interested pre-adolescents be taken into account. Because of this the present writers reëxamined a large amount of their data and prepared a second study which presented data for pre-adolescent and adolescent children. This treatment, which took percentages into account, revealed a remarkable parallelism between the waning of numerous play interests and the onset of pubescence.[5]

[5]H. C. Lehman and Paul Witty, A study of play in relation to pubescence, *J. Soc. Psychol*, 1, 1930, —.

Play activity is of course only one of several behavior manifestations which are associated intimately with and reveal marked changes with the onset of pubescence. This paper presents data which show some interesting relationships between vocational attitudes and the onset of pubescence. These data were secured as follows.

The Lehman Vocational Attitude Quiz was given to more than 25,000 school children in Topeka, Kansas, and in Kansas City, Missouri. This quiz consists of a comprehensive and catholic list of 200 occupations. The children are first asked to check *only* those occupations in which they are willing to engage as a life work. They are then asked to indicate: (1) the three occupations which they would like best to follow; (2) the one occupation which they most likely will follow; (3) the three occupations which they think are the best money-makers; (4) the three occupations which they believe are most respected; and (5) the three occupations which they believe will require the least amount of effort. The number and the ages of pupils from whom data were obtained are presented in Table I.

The data regarding the onset of pubescence in girls were secured by other investigators.[6] Table II shows the age of sexual maturation for 6 groups of American girls; the data are expressed in percentages by a cumulative frequency distribution. Although the onset of pubescence varies somewhat from group to group, individual differences within the various groups are similar. When curves are drawn to show the variability in age of onset of pubescence, one finds all 6 curves are strikingly similar. In so far as the present discussion is concerned, this parallelism is most significant.

Since most of the girls for whom data are presented in Table II were prepubescent until the age 10½, the writers have used 10½ as the basal year and have expressed their data in percentages from age 10½ to 17½. Fig. 1 gives data for girls as regards the percents of those who think they will probably become "movie actresses." At age 10½, 12% of the girls indicated that they expected to become "movie actresses." In Fig. 1 the writers have indicated therefore the percentages of these girls who will probably continue to exhibit this expectation at successive age levels. The writers are here assuming that the present 10½-yr.-old girls will respond next year as do the 11½-yr.-old girls, etc.

In Fig. 1 the solid line represents the percentages of girls who have reached pubescence at various ages. Data for constructing this curve were assembled by Abernathy.[7] Fig. 1 shows that there is a very close relationship between the onset of pubescence and the decline of the girls' written expression of intention to become "movie actresses." This similarity of the curves is particularly noteworthy since the data were assembled during different decades from entirely different groups of girls living in different communities.

Eight graphs were constructed in which the parallelism between a change in the girls' vocational attitude and the onset of pubescence in them was re-

[6] B. T. Baldwin, The physical growth of children from birth to maturity, *Univ. Iowa Studies in Child Welfare*, 1, 1921, (no. 1) 190; E. M. Abernathy, Correlations in the physiological and mental development of high school girls, *Univ. Chicago Master's Thesis*, 1924, 46; R. K. Atkinson, A study of athletic ability of high school girls, *Am. Phys. Educ. Rev.*, 30, 1925, 458-466, 539-546.

[7] Abernathy, *op. cit.*

markably close. The cost of reproducing these graphs makes their publication inadvisable. In Table III, however, data for the 8 occupational attitudes are presented. The first column of Table III shows the percentage of 10½-yr.-old girls who indicated that they would willingly engage in the several occupations. The remaining columns indicate the percentage of these girls who will probably continue to maintain their attitudes at each year beyond age 10½.

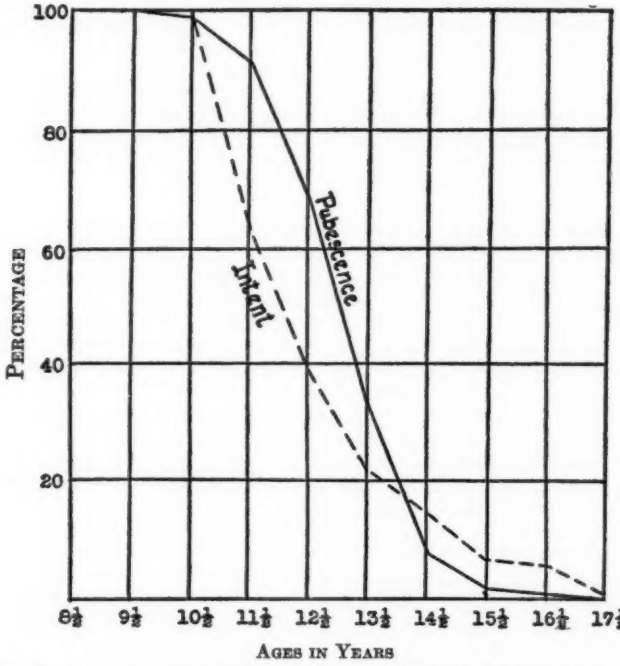

FIG. 1. RELATIONSHIP BETWEEN ONSET OF PUBESCENCE IN GIRLS AND DECLINE OF THEIR WRITTEN EXPRESSION OF INTENTION TO BECOME "MOVIE ACTRESSES"

In Fig. 2 the writers have presented the percentages of 11½-yr.-old boys who indicated that they would *like best* to be "cowboys," and the percentage who continue to exhibit this attitude at successive age levels. In Fig. 2 the solid line represents the percentage of boys who have become pubescent at various age levels. Data for making this curve were assembled by Crampton for 3,835 New York City boys.[8] Although, as previously mentioned, Furfey employed 12 years as the basal age in making his computations, the present

[8]C. W. Crampton, Physiological age: a fundamental principle, *Am. Phys. Educ. Rev.*, 13, 1908, 141-154, 214-227, 268-283, 345-350.

TABLE II

AGE OF SEXUAL MATURING FOR 6 GROUPS OF AMERICAN GIRLS
(Percentage cumulative frequency distribution)

	Group*					
	1	2	3	4	5	6
Ages	487	6875	47	151	56	134
9½						
10						
10½	1.03					2.23
11						
11½	8.63		8.51	4.63	5.35	9.69
12		3.20				
12½	31.62		27.65	21.18	21.41	29.83
13		18.50				
13½	66.94		65.94	58.26	60.69	59.67
14		58.00				
14½	92.20		87.21	86.07	85.69	86.52
15		89.40				
15½	98.16		99.97	97.32	96.40	96.22
16		98.70				
16½	99.79			99.96	99.97	99.94
17		100.00				

*Group 1 is from Abernathy's study; Group 2 from Atkinson's; and Groups 3-6 are from Baldwin's (cf. footnote 6 above).

TABLE III

VOCATIONS TOWARDS WHICH GIRLS MANIFEST A CHANGE IN ATTITUDE THAT PARALLELS RATHER CLOSELY THE ONSET OF PUBESCENCE

Vocation	Ages							
	10½	11½	12½	13½	14½	15½	16½	17½
Magician	100%	72%	45%	32%	26%	7%	8%	1%
Circus performer	100	80	56	47	32	18	11	6
Jeweler or watchmaker	100	72	44	34	24	17	20	19
Maid or servant	100	74	49	42	22	19	21	13
*Movie actor or actress	100	72	57	43	32	18	15	10
†Movie actor or actress	100	63	39	22	15	7	6	1
‡Teacher in grades or rural schools	100	77	53	37	26	17	11	6
§Nurse	100	72	61	48	31	15	13	8

*Activities which the girls state that they *would like best* to follow.
†Activities which the girls say they most likely *will* follow.
‡Activities which the girls believe to be the *best money-makers.*
§Activities which the girls believe are *most respected.*

writers have concluded (in the light of their data) that a younger age is more appropriate for this purpose. It is of course true that behavior manifestations show considerable variability in their waxing and waning. Although the changes of attitude herein studied correspond closely to the onset of pubescence, they do not parallel *exactly* the biological phenomenon. Therefore al-

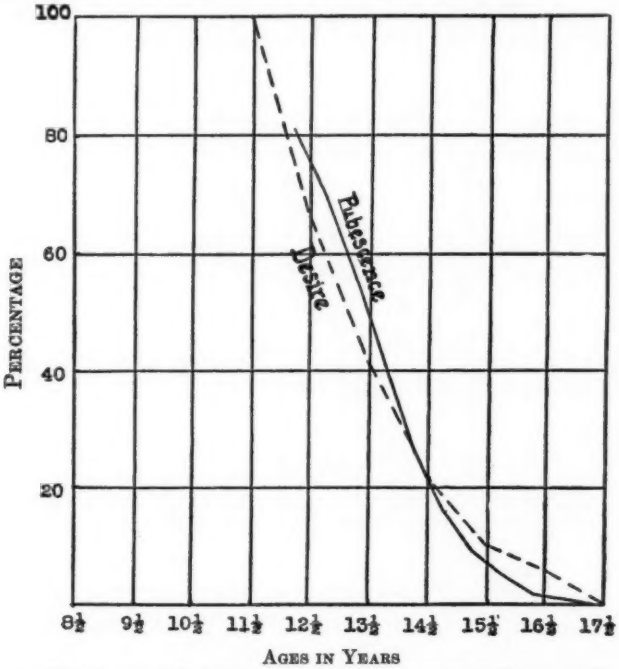

FIG. 2. RELATIONSHIP BETWEEN ONSET OF PUBESCENCE IN BOYS AND DECLINE OF THEIR DESIRE TO BECOME "COWBOYS"

lowance must be made for the variability in these behavior manifestations. The attention of the reader will be attracted nevertheless by the similarity of the various curves and their close correspondence with the curves for the onset of pubescence.

In Table IV, data are presented for 20 vocational attitudes in which *change* in attitude parallels closely the onset of pubescence. In Table IV the writers have used age 11½ as the basal age for the following reason: Crampton's data show that from 15 to 20% of New York City boys become pubescent prior to age 12.[9] It seems illogical therefore to assemble data for 12-yr. old boys and

[9] *Op. cit.*

then to assume that the data represent the attitudes of pre-adolescents. Data originally assembled by the Baltimore Public Athletic League for 3,600 city boys and for 1,317 country boys are presented by B. T. Baldwin.[10] These data reveal clearly that the pubescent curve for boys starts to descend quite perceptibly prior to the age of 12 years.

TABLE IV

VOCATIONS TOWARD WHICH BOYS MANIFEST A CHANGE IN ATTITUDE THAT PARALLELS RATHER CLOSELY THE ONSET OF PUBESCENCE

Vocation	Ages						
	11½	12½	13½	14½	15½	16½	17½
Ship builder	100%	75%	55%	40%	38%	32%	25%
Soldier	100	80	57	34	22	19	14
Sailor	100	85	71	50	36	30	20
Circus performer	100	75	50	25	14	12	12
Brakeman or conductor on a train	100	70	68	49	32	30	23
Switchman or yardman	100	68	57	35	27	16	10
Fireman (answering fire alarms)	100	74	46	33	26	14	9
Motorman	100	63	47	23	19	8	13
Cowboy	100	75	63	43	21	20	9
Sheepherder	100	65	52	29	13	9	6
Night watchman	100	58	37	20	13	16	9
Sheriff or policeman	100	70	47	32	17	13	10
Worker in railroad shops	100	75	59	37	40	28	25
Glass blower	100	68	36	37	16	11	14
Jeweler or watchmaker	100	82	70	47	27	21	23
Drayman, teamster or truck driver	100	57	41	41	23	21	18
Messenger	100	66	50	34	21	11	00
Cowboy*	100	66	41	21	10	6	00
Cowboy†	100	68	23	13	11	0	00
Cowboy‡	100	74	41	37	14	0	00

*Activities which the boys say they would like best to follow.
†Activities which the boys say they most likely will follow.
‡Activities which the boys believe are most respected.

Despite the above facts it is rather dangerous to assume *a priori* that *any* age level is most suitable as a basal age from which to make computations. This becomes apparent when one realizes that onset of pubescence varies from group to group.

[10]B. T. Baldwin, A measuring scale for physical growth and physiological ages, *15th Yearbook Nat. Soc. for the Study of Educ.*, 1916, (Part I, Chap. I, 16).

In Fig. 1 it will be noted that the curve representing attitude rises slightly above the pubescent curve at the right end of the graph (at the higher age levels). This characteristic occurred so frequently in the attitude curves that it deserves special comment. The following hypotheses or some combination of them may explain this tendency. Every occupation is attractive to some children at every age-level. Therefore the attitude curve might well be expected to be slightly higher than the pubescent curve at the higher age levels. Ignorance of occupational opportunities and occupational requirements also may account in part for the fact that in some instances the attitude curve rises considerably above the pubescent curve at the older age levels. Unselected children were used in the several investigations conducted by the present writers.[11] Therefore, there are of necessity the responses of a rather large number of mediocre and dull children included in the percentage calculations. The mediocre and dull children probably do not discriminate very successfully among the occupations. For this reason one might anticipate less accurate results (with some overstatement) at the higher age levels. The questionary itself might be expected to have the common limitations of such techniques. All the factors mentioned above probably combine to affect the curves.

Further Remarks

Probably all will agree that the responses of children in the lower grades regarding occupational endeavor are inaccurate and often unintelligent. The children are grossly ignorant regarding the financial remuneration of various vocations, and the amount of effort demanded by them. Nevertheless, the data show that by the time children reach the third or fourth grade in school they have already developed definite *attitudes* toward certain vocations. Much of their vocational knowledge has been acquired informally. Some of it may have been due to attendance at the movies, perusal of magazines and books, and listening to adult conversation. The extent to which the early attitudes of children are based on actual *knowledge* is unknown. It seems that affective responses rather than knowledge determine early vocational attitude. Nevertheless, definite and pronounced vocational attitudes appear early in children.

The data presented show also that with the onset of pubescence, a marked change in occupational attitude takes place. This is especially marked in certain occupations. Although, as mentioned by Furfey, it is hazardous to generalize too freely with regard to such findings, one must recognize that the onset of pubescence is intimately associated with the change of attitude.

It is difficult to adduce unequivocal evidence showing that the onset of pubescence is a *cause* of the modified attitudes. The children themselves could hardly be expected to explain their changed outlook. Questioning the children as to their "reasons" for changing their minds would yield partial and unreliable explanations only. Interesting in this connection is the list of 'reasons' advanced by girls for ceasing to play with dolls. The following 'reasons' were assembled by A. C. Ellis and G. Stanley Hall.

"In the supplementary paper 55 stopped playing with dolls because they liked other things better; 50 ceased to care for them without being able to give a reason; 45 stopped because they were too old; 44 because too large; 22 be-

[11]The children were unselected except for the fact that the school tends to retain those who are most interested in the type of work it has to offer.

cause too busy and had no time; 15 because ashamed; 11 because love a real baby. Others gave their dolls away; preferred new playmates; were made to stop; dolls were worn out."[12]

It is true that the girls' loss of interest in doll play parallels closely the onset of sexual maturation. The writers believe therefore that the above 'reasons' are largely rationalization; the girls themselves are probably unaware of the fact that their loss of interest in dolls is closely associated with sexual maturation.

The attitude of children toward certain play activities is probably paralleled in certain of their vocational attitudes. Very young children could probably supply 'reasons' for their early vocational preferences. The older children could probably supply satisfactory 'reasons' for their modified attitudes. But to the student of psychology it must be clear that these changes in attitude parallel closely sexual maturation and are probably due in part to this biological phenomenon. It would be folly to argue that these modifications in attitude are due *wholly* to physiological maturation. One should keep in mind that the children studied are American city children living in the 20th century. The maturation of vocational attitudes is undoubtedly influenced by the attitudes of adults who live in the communities studied. It is doubtful whether children living in a pastoral civilization would manifest such change in attitude as that shown herein toward "cowboy." One fundamental fact disclosed by the present study is that the children's vocational attitudes mature relatively rapidly after the onset of pubescence.[13] *What* the adult attitude will be toward specific occupations is of course dependent in part upon the community in which a child lives. *Which* attitude will be acquired seems to be largely a matter of environment.

The foregoing data have a direct bearing also upon the problem of the relation of interest to ability. It is sometimes asserted that interests are symptomatic of ability. Ability is usually conceived as inherent capacity, *i.e.* capacity inborn and unchangeable. If the latter conception of ability be adhered to, the hypothesis that interests are symptomatic of ability postulates the additional hypothesis that interests do not change. If, however, the written expression of intention to enter a given vocation be regarded as an expression of interest, it is obvious that the interests of the children herein studied cannot be symptomatic of their abilities, for the interests of these children are found to change markedly during the pubescent period. This fact is of especial significance to the vocational counsellor. To the extent that children's vocational attitudes change with pubescence, the utmost discretion will be necessary when vocational counsel is given to the pre-adolescent. Perhaps such counsel should consist of little more than attempts to assist the child in becoming acquainted with the vocational world.

Whether the foregoing hypotheses be valid or not, the data presented in Tables III and IV should be of much value to the student of adolescence, to the vocational counsellor, and to those interested in the psychology of interest.

[12]A. C. Ellis and G. S. Hall, A study of dolls, *Ped. Sem.*, 4, 1896, 156.

[13]It should be borne in mind that the foregoing data were assembled for *groups* of children. The problem of whether a given child experiences *sudden* changes in vocational attitude is one that the writers have not investigated.

THE RETROACTIVE EFFECT OF PLEASANT AND UNPLEASANT ODORS ON LEARNING

By J. D. Frank and E. J. Ludvigh, Harvard University

It has often been held that pleasantness and unpleasantness acting retroactively are important determining factors in learning. There has been, so far as the authors know, only one experiment bearing on this point.[1] Since the methodology of that experiment was designed primarily for the investigation of a different problem, it is not finally conclusive as to the retroactive effect of pleasantness and unpleasantness. There was read to *S* a list of neutral words; afterwards he read some pleasant or unpleasant material, and then recall for the neutral words was tested. Certain difficulties are obvious. In the first place, due to inevitable associations, it is impossible to make up a word list that is neutral for everyone. A list may be pleasant for some *Ss* and unpleasant for others. The same objection can be raised in regard to the materials. Certainly the degree of affectivity of such materials varies greatly for different *Ss*. In the second place, Tait does not say whether a given *S* was tested after both pleasant and unpleasant stimulation or whether some *Ss* were given pleasant and others unpleasant stimulation. If the former, then the effect of the learning curve is neglected; if the latter, we are ignorant as to whether Tait used the identical neutral list for the different *Ss*. Since Tait left these vital points unsettled, we feel justified in regarding his results as inconclusive.

The present investigation is an attempt to verify experimentally the alleged retroactive effect of pleasantness and unpleasantness on learning. It was designed especially to avoid variation of attentional and emotional factors in affective stimulation and to keep extraneous factors as constant as possible. These factors were reduced to a minimum by limiting the entire experiment to two hours and by giving a control test at the beginning and end of the experiment. In this way we hoped that errors due to the factors of fatigue, practice, and so forth would largely compensate for each other. The emotional and attentional factors were kept constant by giving pleasant, unpleasant and indifferent stimulations in exactly the same way. In this experiment 15 *Ss* were used, all university students. Every *S* was given the following directions:

*Accepted for publication April 14, 1930.

[1] W. D. Tait, The effect of psycho-physical attitudes on memory, *J. Abn. & Soc. Psychol.*, 8, 1913, 10-17. Although there have been experiments on animals dealing with this point (J. B. Watson, The effect of delayed feeding upon learning; *Psychobiol.*, 1, 1917, 51-59; C. J. Warden and E. L. Haas, *J. Comp. Psychol.*, 7, 1927, 107-161), whether or not animals experience pleasantness and unpleasantness as conscious states remains a matter of controversy. Hence it was felt that an investigation of this problem using human beings, who can report directly on their affective states, might prove of value.

"*Please read carefully*

"You will be shown a list of 10 pairs of meaningless syllables, one pair at a time. These syllables you are to memorize, so that, upon presentation of the first syllable of a pair, you will be able to give the second.[2] Make no attempt to memorize the list as a whole.

"Each pair of syllables will be shown for about 2 sec. at a time. The entire list will be repeated 10 times.

"In order to facilitate the learning, *repeat the syllables aloud* in pairs as they are presented, emphasizing the first of each pair so as to get a rhythm. You may use any supplementary form of learning you wish.

"Concentrate entirely on the pair of syllables before your eyes.

"Immediately after the last presentation of the syllables, you will be given a series of odors to judge. For each judgment two odors will be presented, one after the other. If you prefer the first of the pair, indicate by saying 'first;' if you prefer the second, say 'second.' In addition, *as each odor is presented*, please tell whether you consider it very pleasant, pleasant, indifferent, unpleasant, or very unpleasant."

The syllables were presented by means of the Marx memory apparatus. A cardboard screen, having a slit through which the syllables were presented, hid the *Es* from the *Ss*. About 30 sec. after the presentation of the syllables, the first odor was presented. The period of time over which the odors were presented was almost identical in every case. There were 6 odors and 15 comparisons in each stimulation group.

The odors were divided into 'pleasant,' 'unpleasant,' and 'indifferent' presentation groups, in accordance with the results from other experiments involving a total of 10 *Ss*. The general affective rank order of the odors as ascertained from an average of our *Ss*' rank-order judgments coincided almost exactly with the one upon the basis of which the division into presentation groups was made. Hence it may be assumed that the stimulations produced the desired affective states.

After the first presentation *S* was given the six 'indifferent' odors, determined as above, to judge as the directions indicate. These judgments were scored as follows: very pleasant, +2; pleasant, +1; indifferent, 0; unpleasant, −1; very unpleasant, −2. From these figures a rank order of the stimuli according to absolute judgments was obtained. *S* was then given a 10-min. rest period during which he was supplied with back copies of the *Nation* for reading matter. Then his recall was tested. The following directions were given for the recall:

"*Please read carefully*

"In the list of syllables you studied a few minutes ago, the first syllable of each pair is known as the question syllable, the second as the answer syllable. The question syllables will now be presented to you in random order, and you are to give the *correct* answer syllables as rapidly as possible.

"Only one question syllable will be shown at a time, and each syllable will be shown only once. The question syllable will be withdrawn and another one substituted for it in the following cases:

"(1) When you give an answer syllable, whether right or wrong.

"(2) As soon as you let us know that you are certain you can not think of the proper answer syllable. As soon as you are convinced that you can not think of the proper answer syllable, say 'I quit.' *Do not give up though, until you are certain that further effort is useless.*

[2]This method is essentially that of right associates (Treffermethode) as developed by G. E. Müller. Cf. G. E. Müller, and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtnis, *Zsch. f. Psychol.*, Erg. Bd. I, 1900, 194 ff.

"If you can not give the exact answer syllable, but can give an approximation, do so. The approximation will be counted as a response and the question syllable changed as above."

The method of scoring the responses was adapted from Müller. A syllable entirely correct scored 3 points. Two consonants or a consonant and vowel correct in the proper order scored 2 points. A consonant correct and in the proper position scored 1 point. Half credit, 1½ points, was allowed for an interchange of responses, *e.g.* if two of the presented pairs of syllables were *dak*, *tov*; *zan*, *bef*; and *S* responded to *dak* with *bef* and to *zan* with *tov* he was credited with 1½ points for each response, or 3 points in all. All else scored nothing.

There were four lists of syllables in all. For every *S* the lists were presented in random order, in order to nullify any effect that a particularly easy or difficult list might have on the results. The total score for any list for any *S* is the sum of the response scores in that list.

After the recall *S* was given 5 min. "to forget all about the list you have just had." He was then given another presentation of a different list of syllables and the procedure was repeated as above, except that 'pleasant' odors were substituted for 'indifferent' ones. The procedure was again repeated with 'unpleasant' odors and then with the original 'indifferent' ones. With half of the *S*s the procedure was reversed;[3] the 'unpleasant' odors were given second and the 'pleasant' third. At the end of the experiment every *S* was asked to arrange the odors in order from the most pleasant to the least, and to divide the odors into three groups, pleasant, unpleasant, and indifferent. This procedure provided a rank order of the odors for every *S* as determined by the order-of-merit method.

For persons desiring to repeat this experiment it may be pointed out that careful control of the conditions is highly desirable. Thus the interval between the end of the learning and the beginning of the stimulation must be accurately timed. Furthermore, the period of stimulation must be kept constant in time as well as in number of pairs of odors presented. The period between the end of the stimulation and the beginning of the recall must also be kept strictly constant. The first period mentioned should vary not more than 3 sec., the second not more than 10, and the third not more than 15 sec. for every *S*. The activities of *S* between the periods of learning and recall should be kept as uniform as possible. It cannot be too strongly emphasized that, in our opinion, this experiment deals with the complex reaction of an organism to a situation which must be considered in its totality. Hence a variation or inconstancy in any part of the procedure may markedly affect the validity of the results.

In the present experiment the effect of learning was nullified as far as possible both by giving an indifferent stimulation first and last, and by interchanging the positions of pleasant and unpleasant stimulations. It would be desirable to eliminate the influence of learning as entirely as possible, by giving the *S* enough practice periods to flatten out the learning curve before the start of the

[3]As explained below it was found advisable to disregard certain *S*s in the presentation of final results. The statement in the text applies to those 12 *S*s that were used in the complete statistical calculations of these results.

actual experiment. Unfortunately, this improvement in methodology was not thought of until most of the results had been obtained, and it could not be used.

Table I gives a complete summary of the data obtained. The figures in the column for response-score represent the total score for the nonsense-syllable list. The total score for any list for any *S* is the sum of the response-scores in that list. The figures in the columns marked "sum of the affective judgments" represent the total affective score for the stimulation group. The total affective score for any stimulation group for any *S* is the algebraic sum of the *S*'s absolute judgments on the individual odors. The numbers 1, 2, 3, and 4 above the columns indicate the temporal sequence of the 'response-scores' and the 'affective-judgment' scores. The 'response-scores' in each column were obtained after the 'affective-judgment' score in the corresponding column.

TABLE I

NONSENSE SYLLABLE SCORES FOLLOWING 'INDIFFERENT,' 'UNPLEASANT' AND 'PLEASANT' ODOR SERIES, AND THE SUM OF EACH *S*'S AFFECTIVE JUDGMENTS FOR EACH OF THE SERIES

Subject	Response Scores				Sum of Affective Judgments			
	1	2	3	4	1	2	3	4
21	13	8	22	10	− 7	−26	10	− 9
23	7	6	9	5	21	− 8	28	30
*26	4	9	6	—	− 7	−16	5	—
28	14	9	19	11	− 2	−40	− 7	−11
29	18	14	17	16	2	− 3	20	8
31	12	13	24	18	7	−38	29	9
32	18	19	20	16	16	9	19	26
*22	7	13	—	—	10	− 7	—	—
24	11	17	15	21	− 4	18	−21	13
*25	10	12	13	5	18	14	−21	7
27	19	19	13	25	9	9	−12	20
30	6	8	8	11	− 5	26	−26	23
33	21	26	30	28	0	15	− 2	9
34	19	8	4	17	− 3	− 7	−35	− 4
35	17	22	15	17	−18	− 6	−24	−21

For every *S* an indifferent stimulation group was given first and last (columns 1 and 4 respectively). All *S*s above the line through the middle of the table were given the unpleasant stimulation before the pleasant stimulation (columns 2 and 3 respectively). For all *S*s below the line the pleasant stimulation was given before the unpleasant stimulation (columns 2 and 3 respectively).

It will be noted that in the first half of Table I (2nd list followed by unpleasant stimulation, 3rd by pleasant stimulation) 6 out of 7 *S*s show higher scores for the 3rd list than for the 2nd; and in the second half of the table (2nd list followed by pleasant stimulation, 3rd by unpleasant stimulation) 4 out of 7 *S*s show higher scores for the 2nd list than for the 3rd,[4] and one shows equal scores. It is quite apparent then, even in these rough data, that the pleasant odors tended to be followed by better responses than the unpleasant ones.

[4]Subject 22 cannot be considered here because his third response-score, as well as his fourth, were unobtainable (see text).

In order, however, to get an adequate impression of the results, it is necessary to omit the *Ss* who have been 'starred' (asterisk in the table) for the reasons given below.

With *S* 22, the presentation apparatus broke down immediately after the second recall, and no significant data could be obtained.

S 25 smoked after the unpleasant presentation. As he was an habitual smoker and had not smoked for a considerable time that day, the smoking may be assumed to have had a pleasant effect. Furthermore, since the smoking affected the same sense modality as the previous odors, the pleasantness was even more marked.

Due to a blunder on the part of the *Es* which resulted in a serious inconstancy in the first stimulation, we decided to disregard *S* 26 at once. As a matter of interest, however, the experiment was partially completed, although the fourth test is missing.

TABLE II

RESPONSE SCORES FOR 12 *Ss* AFTER P,U, AND I STIMULATIONS

Subject	P	Average I	U
21	22	11.5	8
23	9	6.0	6
24	17	16.0	15
27	19	22.0	13
28	19	12.5	9
29	17	17.0	14
30	8	8.5	8
31	24	15.0	13
32	20	17.0	19
33	26	24.5	30
34	8	18.0	4
35	22	17.0	15
Total	211	185	154
% of Average I	114.0%	100%	83.2%

Table II contains the response-scores for the *Ss* other than those starred in Table I. The scores after the two indifferent stimulation groups for every *S* have been averaged. In the table the column headings P, U, and I are to be interpreted as follows: P indicates scores after pleasant stimulation; I indicates score after indifferent stimulation; U indicates score after unpleasant stimulation.

If we total the response-scores as above for the 6 *Ss* who received pleasant stimulation first, we find that the totals are as follows: P = 100, Average I = 106, and U = 85. If we do the same for the 6 *Ss* who received unpleasant stimulation first the totals are: P = 111, Average I = 79, and U = 69.

The fact that the sum of the scores after pleasant stimulation remains greater than the sum of those after unpleasant stimulation, regardless of which stimulation was presented first, seems to indicate that the effect of pleasant and unpleasant stimulation on the scores is greater than that of the learning curve, should the latter be operative in this experiment. This effect is more pronounced if we consider *S*'s P and U scores as percentages of his I score.

The extent to which individual results accord with average results is shown in Table III. The general trend is obviously for the scores after pleasant stimulation to be above those after indifferent stimulation, and for the latter to be above those after unpleasant stimulation. In the first column is indicated the number of *Ss* whose scores are with or against this trend. The second column indicates that the sum of the score-differences for those *Ss* who correspond to the general trend is much greater than the sum of the score-differences in those who do not. Differences are taken as from the average of all four response-scores for each *S*. In the third column a more significant statement of the results is obtained by weighting the score differences. This weighting is achieved by dividing the change in score by the average of all four scores.

TABLE III

RESPONSE SCORES FOR 12 *Ss* SHOWING THE TREND TOWARD HIGHER SCORES FOLLOWING 'PLEASANT' STIMULATION AND TOWARD LOWER SCORES FOLLOWING 'UNPLEASANT' STIMULATION

Results	No. of Subjects	Sum of Score Differences	Sum of Weighted Differences
P above average I	8	39.5	2.83
Average I above P	3	13.5	.30
Average I tied with P	1	0.	0.
U below average I	9	38.5	2.70
Average I below U	2	7.5	.32
Average I tied with U	1	0.	0.
P above U	10	61.	4.31
U above P	1	4.	.15
U tied with P	1	0.	0.

A more detailed statement of the results in terms of coefficients of correlation appears in Table IV. All coefficients were calculated by the Spearman method[5] from the rank orders of all the trials according to affective value and according to weighted score value. Thus the resulting correlations are between the rank order of affectivity of the stimulation groups and the rank order of the weighted score values. The differences (d^2) were taken between the rank of every stimulation group and the rank of every weighted score value following its stimulation group. Thus we have a measure of the relation between affection and subsequent learning.

The correlations involving pleasant and unpleasant stimulation-groups alone are much higher than those involving indifferent stimulation-groups as well. To what extent this is due to the influence of the psychometric function is an interesting question,[6] though one which is beyond the scope of this paper.

The following conclusions seem to be amply warranted. (1) Pleasant odors have a retroactive facilitative effect on learning; unpleasant odors have a

[5] $r = 1 - 6\Sigma(d^2)/n(n^2-1)$.

[6] Cf. E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280-293.

TABLE IV

COEFFICIENTS OF CORRELATION

Correlation between rank according to weighted response changes and	r	P.E.	r/P.E.
(Ia) Rank according to affectivity by absolute judgment	0.52	.071	7.32
(Ib) Rank according to affectivity by rank-order judgment*	0.50	.073	6.85
(Ic) Rank according to affectivity by average rank-order judgments of group†	0.55	.068	8.09
(IIa) Rank according to affectivity by absolute judgments of pleasant and unpleasant groups of odors alone	0.72	.066	10.91
(IIb) Rank according to affectivity by rank-order judgments of pleasant and unpleasant groups of odors alone	0.62	.085	7.29
(IIc) Rank according to affectivity by average rank-order judgments of group on pleasant and unpleasant groups of odors alone	0.67	.076	8.82

*To obtain a rank order of affectivity by rank-order judgments: for each *S* each odor was given a number from 1 (maximally pleasant) to 18 (maximally unpleasant) depending on the position given to it by *S* in his rank-order judgment. The six numbers assigned to the odors in the 'pleasant' group for each *S* were then totalled, as were the numbers assigned to the 'indifferent' and 'unpleasant' groups. There were thus three different totals for each *S*, one for each of the odor groups. These totals for all *S*s were arranged in a rank order according to size; the smallest was placed first. In establishing this rank order, the total of the 'indifferent' group was used twice, since for each *S* there were two response scores corresponding to it.

†To get this rank order, the totals obtained from all *S*s for the 'pleasant' group were averaged. The totals obtained for the other two groups were treated similarly. This procedure gave three numbers, one for each group. These numbers were arranged in a rank order which was then expanded to the necessary length.

retroactive inhibitory effect on learning. (2) The degree of retroactive facilitative and inhibitory effect is proportional to the degree of pleasantness and unpleasantness of the odors as judged by *S*.

These conclusions may also be stated in terms of covariance: the degree of learning varies with the degree of pleasantness and unpleasantness of the odors as judged by *S* in such a way that the greater the pleasantness, the better the learning, and the greater the unpleasantness, the poorer the learning.

It seems reasonable to infer that pleasant and unpleasant stimuli in general have this retroactive effect. This generalization, however, needs substantiation. A further question as to whether pleasantness and unpleasantness *per se*, as opposed to pleasant and unpleasant stimuli, have this retroactive effect, cannot be answered at present. This problem is now being investigated. A third problem consists in the ascertainment of a more exact functional quantitative relation between pleasant and unpleasant stimulation and learning. The data of this experiment are not extensive enough to warrant any conclusions on this point but they indicate that the relation is not of a simple linear type. This problem is also under investigation.

THE EFFECT OF COLOR ON APPARENT SIZE

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In a series of studies culminating in that of Taylor,[1] it has been shown that when visual perception alone is used, color does effect apparent weight; but that when visual-plus-kinaesthetic perception is employed the effect of color on perceived weight is varying and, in general, produces equivocal results. Few experiments have concerned themselves with the effect of color on apparent size of objects. In a recent investigation Warden and Flynn[2] placed eight rectangular cartons identical in size but differing in color on a shelf in a glass case. The first serial arrangement of the colors was determined by chance, and the subsequent seven arrangements by systematically rotating the boxes from left to right. Six college students wrote opposite the name of a color their judgment of its relative size in each of the eight arrangements. The results showed that for any single arrangement of the stimuli there was an effect of color on apparent size, but when all arrangements were considered together the average ranks for the 8 colors were all very close to each other (the range was from 4.2 to 4.9). The authors concluded that the color-size illusion depended in their experiment upon the specific serial arrangement of the boxes rather than upon the quality of the color or the contrast effect of the immediate context.

The present investigation is an attempt to check Warden and Flynn's results (1) by duplicating their method, and (2) by using a different method. In Part I the stimuli and the method of conducting the experiment were the same as Warden and Flynn's with the exception that Milton Bradley instead of Hering papers were employed, and the stimuli were exposed on an open shelf instead of in a glass-fronted case. The shelf and background were neutral gray. In Part II, the colored objects were judged for apparent size by the method of paired comparisons. For every *S*, on the basis of these data, a ranking for effect of color on apparent weight was made. In our discussion, rank 1 signifies the colored object that appeared largest. Thirty-five university students (12 men and 23 women) served as *S*s in both parts of the experiment. Part I was completed at the first sitting, and Part II at a second sitting 7 to 10 days later.

The results of Part I and a summary of Warden and Flynn's results are given in the upper parts of Table I. The figures are in terms of ranks. That is, in row 1, the average rank assigned to gray by 35 *S*s was 4.57, with a standard deviation of 2.5 (ranks). It is evident that the effect of color on apparent size is present to a conspicuous degree under every arrangement. Contrary to

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[1]C. D. Taylor, Visual perception versus visual plus kinaesthetic perception in judging colored weights, *J. Gen. Psychol.*, (in press).

[2]C. J. Warden and E. L. Flynn, The effect of color on apparent size and weight, this JOURNAL, 37, 1926, 398-401.

Warden and Flynn's findings, our results vary but little from arrangement to arrangement. Inspection of each of the successive columns reveals a marked agreement in the average ranks of any color from arrangement to arrangement. The standard deviation of the distribution of the 8 averages for each color is given in the 11th row of figures and the total average (8 arrangements) for

TABLE I

THE EFFECT OF COLOR ON THE APPARENT RELATIVE SIZE OF OBJECTS

(35 *Ss*)

	Arrangement	Gray	Yellow	Blue	White	Black	Green	Red	Purple
Part I: Rank Order	A	4.57	3.58	5.20	4.40	6.68	3.40	4.00	4.34
	[S.D.dis.	2.5	2.16	1.47	2.28	2.32	1.74	2.10	1.91]
	B	4.37	3.86	4.79	4.46	6.80	3.92	4.14	3.71
	C	4.97	2.91	4.06	3.80	6.54	4.57	5.14	4.31
	D	5.31	2.57	4.00	3.52	6.09	4.03	5.86	4.63
	E	5.97	2.74	4.94	3.03	5.20	4.42	6.14	4.74
	F	5.40	2.69	3.91	3.51	6.09	3.71	5.80	4.99
	G	5.49	2.99	4.66	3.80	6.37	2.97	5.29	4.32
	H	5.20	3.08	4.80	3.91	6.80	3.57	4.57	4.06
	Total Av.	5.16	3.05	4.54	3.80	6.32	3.82	5.12	4.39
	S. D.dis.	.48	.42	.45	.44	.45	.49	.76	.37
	Rank	7	1	5	2	8	3	6	4
Results of W. & F.	Total Av.	4.50	4.90	4.80	4.90	4.30	4.30	4.40	4.20
	S. D.dis.	1.46	1.33	1.21	1.57	1.46	1.06	.47	1.30
	Rank	4	1.5	3	1.5	6.5	6.5	5	8
Part II: Paired Comp.	Average	4.99	2.13	3.88	2.32	5.48	1.75	4.56	2.90
	S. D.dis.	1.56	2.05	1.51	1.57	1.93	1.58	1.84	1.81
	Rank	7	2	5	3	8	1	6	4

each color in row 10. For comparison, Warden and Flynn's total averages are given in row 13. The contrast is obvious. While Warden and Flynn's averages show no consistent effect of color on apparent size when all arrangements are considered together, the present experiment reveals a marked effect.

In Part II (method of paired comparisons) the results, given in the lower section of the table, show approximately the same tendencies as those in Part I. Correlation of the ranking in Part I with ranking in Part II yields a coefficient of +0.93. The ranking of colors in Warden and Flynn's study show little agreement with either of our rankings. It is possible that not enough *Ss* were employed by them to produce reliable results. In Part II, the rankings derived from 17 *Ss* correlated +0.91 with that from the remaining 18 *Ss*. This indicates that the ranking for apparent size has been established with a high degree of reliability.

The standard deviations in the 17th row are to be compared with those in the 2nd row.

The results obtained show that light objects appear comparatively large and dark objects comparatively small. When the ranking in Part II is correlated with the luminosities (luminosity of white, 100; black, 0) of the colors a coefficient of +0.86 results. This indicates a rather marked correspondence between the effect of color on size and luminosity.

Summary

(1) A repetition of Warden and Flynn's study of the effect of color on apparent size failed to confirm their findings. Results in two methods (order of merit and paired comparisons) agree with each other but refute Warden and Flynn's findings.

(2) The results of the present study reveal a marked and consistent effect of color on apparent size and this effect appears to be directly related to luminosity of the colors involved.

MINOR STUDY FROM THE PSYCHOLOGICAL LABORATORY OF PRINCETON UNIVERSITY

IV. Variations in the Illusion of Filled and Unfilled Tactual Space

By Frederick E. Craig, Princeton University

The study of filled and unfilled tactual space has yielded various results. With the method of passive stimulation, Volkman,[1] Fechner,[2] James,[3] and Nichols[4] found that a series of points was judged shorter than two points covering an objectively equal extent. Parrish used various numbers of points and generally found that a greater number of points appeared to cover a relatively smaller extent than fewer points.[5] Cook obtained results similar to those of Nichols and Parrish when using their method of presenting the filled and unfilled stimuli successively on the same cutaneous area, but when presenting the stimuli successively on different areas, the filled space appeared equal.[6]

The purpose of this experiment was to determine the 2-point tactual extent which appeared equal to a filled tactual extent of 2¼ in. containing 10 points, each ¼ in. apart; also to determine the 2-point tactual extent which appeared equal to a partially filled tactual extent of 2¼ in. containing 4 points of which 2 were at each end, ¼ in. apart, with a gap of 1¾ in. between the pairs. The stimuli were presented in successive order. The standard stimuli were always presented before the 2-point variable. To check the possibility of a time-error in this order of presentation, a comparison was also made between a 2-point standard (with 2¼ in. between the points) and the 2-point variable. Thus, 3 standards, the 10-point, the 4-point, and the 2-point, were compared with the 2-point variable. The method of limits was used. The variable was increased from a minimal extent of 1¼ in. and decreased from a maximal extent of 3¼ in.

For the 2-point stimulus, an enlarged form of the Langfeld and Allport cardboard aesthesiometer was used.[7] The standard 10-point stimulus consisted of a stiff card with saw-tooth points cut along one edge; the standard 4-point stimulus was identical with the 10-point standard except that the middle 6 points were cut out; the standard 2-point stimulus was identical with the 10-point standard except that the middle 8 points were cut out.

*Accepted for publication May 28, 1930.

[1]See G. T. Fechner, *Elemente der Psychophysik*, 1860, II, 328.

[2]*Ibid.*

[3]W. James, *Principles of Psychology*, 1890, II, 141-2.

[4]H. Nichols, *Our Notions of Number and Space*, 1894, 97 ff.

[5]C. S. Parrish, The cutaneous estimation of open and filled space, this Journal, 6, 1895, 514-520.

[6]H. D. Cook, Die taktile Schätzung von ausgefüllten und leeren Strecken, *Arch. f. d. ges. Psychol.*, 16, 1910, 418-548.

[7]H. S. Langfeld and F. H. Allport, *An Elementary Laboratory Course in Psychology*, 1916, 65.

TABLE I

SHOWING FOR EVERY *S* AND FOR EVERY STANDARD THE LENGTH IN INCHES OF THE TWO-POINT VARIABLE THAT WAS JUDGED AS EQUAL TO IT

(Every value the average of 10 judgments)

Subjects	Standard		
	10-point	4-point	2-point
1	1.99	2.03	2.20
2	1.84	1.91	2.15
3	1.99	2.07	2.12
4	2.06	1.93	2.16
5	1.84	1.89	2.09
6	1.88	1.91	2.33
7	1.91	1.95	2.41
8	1.90	1.93	2.32
9	1.61	1.88	2.24
10	1.79	1.87	1.99
11	1.81	2.00	2.17
12	1.78	2.10	2.17
13	1.86	1.84	2.36
14	2.15	2.03	2.16
15	1.63	1.82	2.23
16	1.58	1.64	2.05
17	1.84	2.11	2.34
18	1.96	2.13	2.19
19	1.83	2.08	2.43
20	1.56	1.88	2.03
Ave.	1.84	1.95	2.21
S. D.	0.15	0.12	0.12
Difference	0.41 in. (18%)	0.30 in. (13%)	0.04 in. (2%)
Significance of diff. between groups	2.56	6.84	

The passive method was used. The stimuli were presented longitudinally on the volar surface of the right forearm. A screen hid the arm from the view of the *S*. The variable was set slightly to the right or left of the place which had just been touched by the standard. The stimuli were presented by hand, care being taken to place all the points of a given stimulus on the skin simultaneously.[8]

There were 20 untrained *S*s (college freshmen). Every *S* was given 6 practice trials and then 30 final trials, all within a single sitting. In the 30 final trials each of the 3 standards was presented 10 times, the variable being increased 5 times and decreased 5 times for each standard. The 30 final trials were presented in haphazard order.

The results for the 20 *S*s are shown in Table I. The numerical values represent the extent of the variable when it was judged equal to a given standard.

[8]C. H. Rieber (*Psychol. Monog.*, 4, 1903, 56) obtained different results with varying degrees of pressure. Such a factor might have altered present results.

From Table I it may be concluded that the 2-point variable was judged on the average to be 18% longer in extent than the filled space, 13% longer than the partially filled, and 2% longer than the unfilled space. For only 3 *Ss* (4, 13, 14) was the average judgment for the partially filled space less than that for the filled space. The 2% difference between the 2-point variable and the 2-point standard may be considered as the measurement of a time-error which would also be present in the other two comparisons. Therefore it may be inferred that the other two percentages would be somewhat reduced if the time error were eliminated.

The above results show a greater difference between the percentages for the 2-point and 4-point standards (2% and 13%) than between those for the 4-point and 10-point standards (13% and 18%). Apparently, the 4 points (two at each end) of the partially filled space have a greater effect than the additional 6 central points in producing the judgment that filled space is shorter than unfilled.

APPARATUS

APPARATUS FOR RECORDING EYE-MOVEMENTS

By MILES A. TINKER, University of Minnesota

Various methods of observation or recording have been employed in the study of eye-movements. These may be grouped into three general types:[1] (1) direct observation; (2) mechanical arrangements for attaching a recording apparatus to the eyeball or eyelid; (3) photographing eye-movements. The main characteristics of eye-movements may be observed by means of any one of the direct methods of observation, *i.e.* by holding a mirror beside the reading text so that *E*, sitting beside the reader, can observe and count the eye-movements. Such methods are crude and yield only an approximate count of the movements. An accurate study of eye-movements in reading requires the use of some mechanical recording device. Methods which involve mechanical attachments to the eyes require very careful techniques to prevent harm to the eyes and to insure satisfactory records. They are consequently limited in their use to a small number of *S*s.

The method of photographing eye-movements developed by Dodge[2] about 1901 permits normal conditions of vision and can be applied to a large number of *S*s. In general the method consists of directing a pencil of light into the eye and photographing the image of this which is reflected into a camera from the surface of the cornea. Modifications of Dodge's technique have recently been employed in investigations at Harvard, Stanford, and Chicago universities. The writer has completed at Minnesota another apparatus for photographing eye-movements.[3]

THE MINNESOTA APPARATUS

A pencil of light from a point source, a carbon arc, is reflected into *S*'s eye by means of a first surface mirror. This beam of light is interrupted at regular intervals of 1/50 (or 1/25) sec. by a perforated revolving disk rotated by a synchronous motor which is controlled by a vibrating bar. The beam of light is reflected from the surface of the cornea through a lens into a long enlarging camera. At the back of the camera the beam is brought to a focus on a kinetoscopic film which moves continuously and at a uniform rate. The direction of

[1]See M. A. Tinker, A photographic study of eye-movements in reading formulae, *Genet. Psychol. Monog.*, 3, 1928, 68-182, for a description of the methods of observing and recording eye-movements.

[2]Tinker, *loc. cit.*

[3]In this discussion only those parts which are new or are important modifications of previous apparatus will be stressed. A more detailed description of the various parts of the Dodge apparatus may be found in Tinker, *loc. cit.*

The Minnesota apparatus was built under the joint auspices of the Psychology and the Educational Psychology Departments. The writer wishes to acknowledge gratefully: (1) the coöperation of Professor Buswell of the University of Chicago who permitted the duplication of parts of their apparatus; (2) the advice of Professor W. R. Miles concerning certain details; and (3) the skilful designing of new parts by R. O. Dauphin of the University shops at Minnesota.

the beam of light reflected from the cornea changes with each movement of the eye. As *S* reads a selection, each fixation is recorded on the film as a sharply focused line. These lines appear as a series of dots rather than solid lines, because the beam of light is interrupted at regular intervals. Each dot represents 1/50 (or 1/25) sec. Inspection of the films yield the number and duration of fixations employed in reading.

The camera and head-rests are mounted on a heavy oak table which is bolted to the concrete floor of a dark-room. The light source and timing device are in an adjoining room and a small hole in the wall admits the beam of light into the dark-room at right angles to the barrel of the camera. The light strikes a mirror placed below the lenses and reading copy and is then reflected upward into the eye.

The light source is a small automatic arc-lamp. This lamp is built by Leitz for use in photo-micrography. The thin carbons yield an admirable light source for our purpose. The rays of light are converged to the mirrors by condensing lenses and the intensity of the light is reduced by passing it through a section of blue glass.

A modification of the synchronous time-marker made by the Cambridge Instrument Company as a part of their electrocardiograph unit is employed as a timing device. A bar, vibrating 50 times per sec., controls a synchronous motor which makes five rotations per sec. A light-weight aluminum disk, 7 in. in diam., is attached directly to the motor in place of the spoked wheel used in the original apparatus. Perforations in the disk permit the light to be interrupted 50 or 25 times per sec. as desired. Twenty-five breaks per sec. produce a decided flicker in the beam of light which is very annoying to *S*. To obtain the clearest break between dot and space on the photograph record the disk should revolve in the opposite direction from that in which the film on the camera is moving.

The head-rest is essentially a circle of steel with padded supports for front, back, and both sides of the head, and both cheeks.

The barrel of the camera, a brass tube 8 in. in diam., is approximately 70 in. long and is mounted on roller bearings which allow the whole camera to be moved forward and backward. At the front of the camera are mounted two Goerz Dagor lenses which are independently adjustable in three dimensions. All adjusting is done by gears operated by *E* from the rear of the camera. The lenses have a 12 in. focus and the distance between lenses and film is 70¾ in. The magnification is about 5. At the back end of the barrel is attached the film-moving device driven by a motor, and the focusing mirrors. The film holders and carriers, which use the standard 35 mm. motion-picture film, are so arranged that the film moves horizontally or vertically, or both. Records on the vertical film represent horizontal eye-movements and those on the horizontal film vertical movements. The film and lens arrangement allow the following possibilities: (1) records from both eyes or from one eye upon either film; (2) records of one eye upon one film and the other eye upon the other film; (3) records from one eye and a reference point (metal bead on spectacle frame) upon either film.

The mirror arrangement attached to the rear of the camera for focusing is shown in Fig. 1. B is the light beam coming from the lens. This strikes the

emulsion surface of the film at F and is reflected to the first surface mirror at M. There are two mirrors at M; one for the horizontal and one for the vertical film. These mirrors are set at such an angle that the beam of light is reflected through the lens at L to the peep-hole at E where *E* views it. Use of this system prevents the waste of any film between records since the rear of the camera is not opened except to insert a new roll of film. Rolls of film 200 feet long are used.

In photographing eye-movements the apparatus should be constructed so that *S* may be as comfortable as possible. A first surfaced mirror 1 in. square, which is ordinarily employed for reflecting the beam of light into the reader's

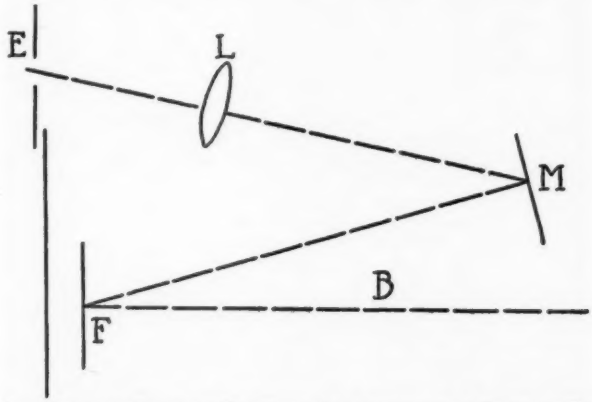

FIG. 1. DIAGRAM SHOWING FOCUSING ARRANGEMENT AT BACK OF CAMERA

eye, floods the eye with a bright illumination which is somewhat annoying. A black mat of paper, with a hole 3/8 in. in diam., placed over this mirror yields light enough for producing a good photographic record and yet causes no discomfort to the reader.

A copy holder for the reading matter is attached to the apparatus at the front of the camera and just below the lenses. The reading material is illuminated indirectly by a tube filled with argon gas. The copy is exposed by the dropping of an automatic shutter controlled from the back of the camera.

To reveal possible head-movements a spectacle frame carrying a bright metal bead is worn by the reader. This yields a record which deviates only when the head moves and thus permits a differentiation between head-movements and eye-movements on the photographic record. It is generally considered that a reference line should be used on all reading records. Fig. 2 shows four records involving varying amounts of head-movements. In each record the left-hand line of dots which is more or less continuous is the reference or head line, and the right or broken line represents eye-movements and pauses. Record 1 reveals practically no head-movements. The sharp, almost hori-

zontal displacements, as at B, A, and C, represent eye-movements. This type of break in the photographic records characterizes eye-movements in reading. In Record 2 there are pronounced head-movements at D and F, but these do not cause any corresponding break in the eye-movement record that might be confused with displacements due to eye-movements such as occur at G and E. In Records 3 and 4 head-movements at M and S produce corresponding *curves* in the eye-movement record but do not cause abrupt horizontal displacements as at H, K, N, and T, which are due to eye-movements. From these records

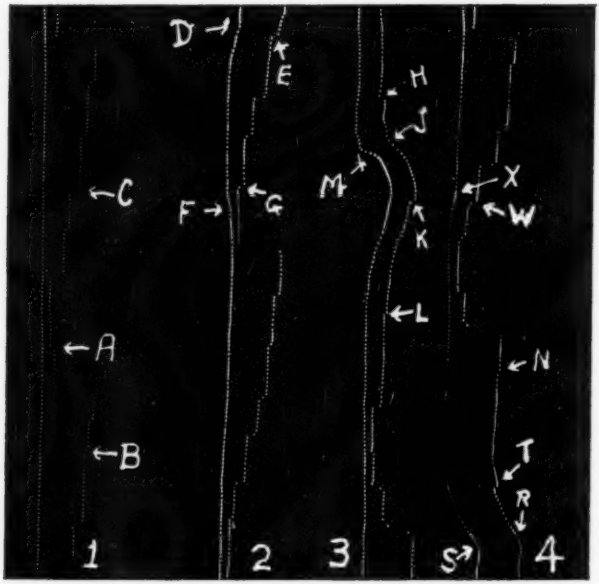

FIG. 2. HEAD-MOVEMENT AND EYE-MOVEMENT RECORDS

it is evident that, even with marked head-movements, there is little possibility of confusing head-movements with eye-movements on the photographic records. With most readers head-movements are no greater than shown on Record 1, and only a very few reveal as marked movements as those represented in Record 2. It is reasonable to conclude from these records that, *in ordinary reading investigations where one is not concerned with relationships between eye-movements and head-movements, there is no need of a head or reference line.* This facilitates the taking of records as only one beam of light needs to be focused.

Reading of the photographic records is greatly facilitated by projecting them upon a screen. The film-slide projector manufactured by Leitz is very satisfactory for this purpose.

THREE NEW PIECES OF APPARATUS FROM THE PSYCHOLOGICAL LABORATORY OF BROWN UNIVERSITY

By Harold Schlosberg and Leonard Carmichael

A Simple Heat Grill

The usual heat grill, maintained at the proper temperature by circulating fluids, is often considered too expensive and cumbersome for use in a large introductory course in experimental psychology. The grill to be described below obviates these difficulties, as well as offering certain advantages of its own.[1]

The new grill consists essentially of two comb-like elements, one of which may be warmed, and the other chilled, by immersion in pans of water. By intermeshing the teeth, and placing them on the surface of the skin, alternate warm and cold rods stimulate the skin, and an experience of heat is usually reported.

The first model of this grill was made by drilling six holes in a row (1 cm. between centers), in each of two blocks of wood (2 × 3 × 8 cm.). Long nails (8 cm.) were forced through these holes, which were just small enough to hold the nails snugly. The nails were then lined up with a pair of pliers, so that they would intermesh with those of the other element. This form of the grill, which has proved itself very satisfactory, can be made by any one who has a few simple tools. A more elaborate model may be made by using blocks of copper (9 × 1.5 × 2 cm.). A row of six holes (13 mm. between centers) should be drilled in the narrow face of each of these blocks. The holes are then tapped and the threaded ends of brass rods (0.5 × 13 cm.) screwed into them. This model holds the desired temperature longer, and offers a larger area for contact with the skin than does the simple 'nail' model. Photographs of both models are shown.

In actual use in the laboratory course, the experimenter places one element in cold water (about 20° C), and the other in warm (about 40° C). He allows several minutes for the elements to come to the temperature of the bath, then removes them and shakes them free of water. The rods are then intermeshed. The *S* then rests his wrist, and later his palm, on the intermeshed alternate warm and cool rods. An experience of heat is almost invariably reported. The *S* sometimes jerks his hand away exclaiming that he has been burned.

Because of ease of construction and simplicity of manipulation, these grills can be supplied to each pair of students in an experimental course. Qualitatively, the results are about as satisfactory as those obtained with a more elaborate grill of the circulating fluid type. Furthermore, each student can experiment for himself, with full knowledge of what he is doing. The fact that the two elements are separated, brought to the proper temperatures, and put together again, gives the students a clearer idea of what has been done than can ordinarily be obtained from the use of the more complicated grill of the circulating-water type.

[1] A preliminary report of this apparatus has been published in the *Proc. Internat. Congr. Psychol.*, 1930, 318 f.

A New Stylus Maze

By making a wooden mold and casting a stylus maze in cement, it is relatively easy to furnish many copies of the same pattern for use in a laboratory course in experimental psychology. This method of production also makes it easy to produce alleys of triangular cross-section. This sort of alley decreases the tendency of the subject to cling to one side of the path, and thus eliminates a constant source of annoyance that is noted by those who use the stylus maze. We have found that the following method is quite successful in producing the mazes described above.

A mold is first made in wood. To a flat board of suitable size strips of wood are nailed in appropriate patterns. These strips should have a right isosceles triangle for their cross-section, and are easily made by ripping down the diagonal of a half-inch square strip. These strips are then cut in a mitre-box into pieces of the shape shown in the accompanying figure. The base of the horizontal members (A) is 3¼ in. in length, and that of the short verticals (B) is 1 in. The cul-de-sac verticals (C) are cut at the opposite angle on one end, and are, therefore, ¾ in. longer, measured at the base. These strips are fastened to a board in a multiple-T pattern. After sandpapering and waxing, and the erection of sides to retain the cement, the maze is cast. A watery solution of pure Portland cement is poured into the mold to the depth of one inch, and reënforced by a sheet of ¼-in. galvanized wire mesh. The cement is allowed to harden for several days, and the mold is then removed. Any small roughnesses in the maze are then smoothed out, and the whole maze is painted with deck paint. After the mold is once made, it is relatively easy to cast any number of duplicate mazes.

As shown in the photograph, the finished maze is a slab of concrete, with V-shaped grooves or alleys forming a continuous path from start to finish. It should be used with a ball-pointed stylus. These styli can be made by rounding off the end of a piece of glass tubing. The way in which the stylus fits in the alley is shown in the accompanying drawing. Due to the shape of the alley and stylus, the *S* maintains contact with both sides of the track continuously, so that he is aware of all possible paths, and cannot miss any blind alleys by pressing against one side only of the alley.

A Simple Apparatus for the Conditioned Reflex

An experiment with the conditioned reflex is an important part of many courses in experimental psychology. The apparatus described below makes it possible to set up conditioned responses in all members of a course with very little difficulty. The response used is the withdrawal of the hand from shock, and the conditioning stimulus is a loud 'buzz.' The following simple device makes it possible to establish and record the process of conditioning. The arrangement utilizes no special equipment, is easy to use, and permits quite accurate measurement of the latent time of response, both conditioned and unconditioned. The following description may be followed more easily by reference to accompanying wiring diagrams.

The primary of an inductorium is arranged in series with a buzzer and two telegraph keys. The buzzer is used in place of the interrupter of the coil,

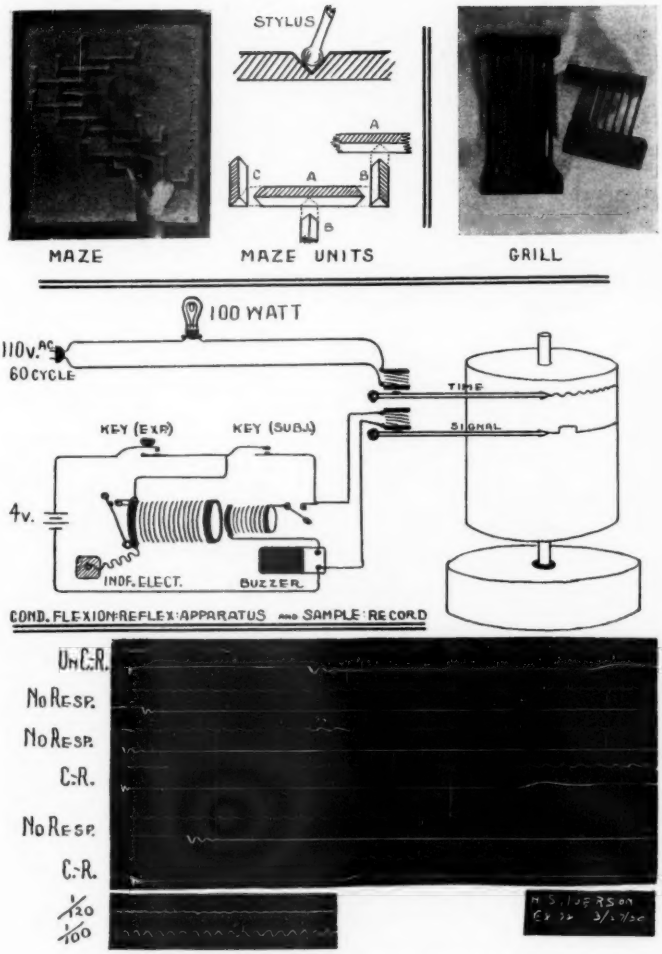

FIG 3. APPARATUS FROM THE PSYCHOLOGICAL LABORATORY OF BROWN UNIVERSITY

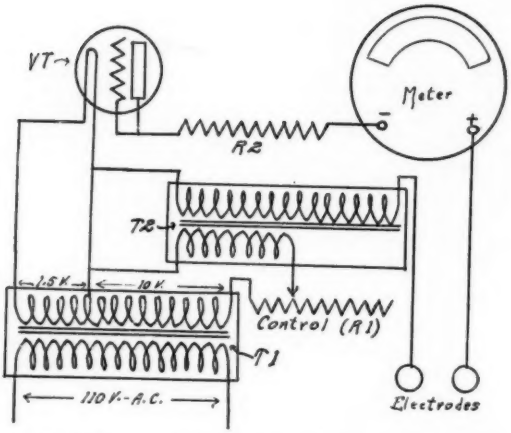

FIG. 4. WIRING DIAGRAM OF THE ELECTRICAL STIMULUS-APPARATUS

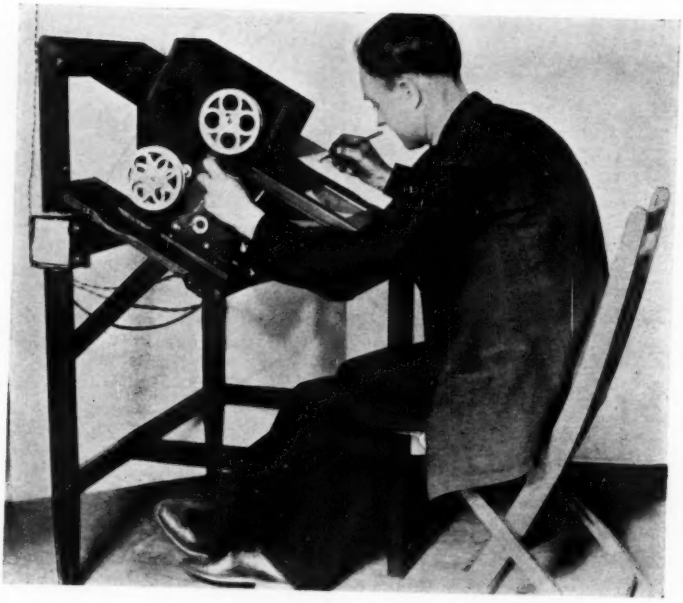

FIG. 5. PROJECTION TABLE AT YALE PSYCHO-CLINIC

and also produces the conditioning sound stimulus. An electric signal-marker is shunted across buzzer and coil. One terminal of the secondary is attached to a large wet 'indifferent' electrode, fastened on the right forearm of the *S*, and the other is attached to the *S*'s key, from which the insulating finger rest is removed. The apparatus is used as follows: *S* presses his key with his right forefinger, and holds it down in preparation for the stimulus. Then *E* completes the circuit by pressing his key. The buzzer sounds, the signal marker is deflected, and *S*'s forefinger is stimulated with a shock. When reflex withdrawal occurs, *S*'s key is necessarily released, opening all circuits. The marker returns to its base line. Thus the excursion of the marker records the time elapsing between stimulus and response. To elicit the conditioned withdrawal, the shock is diverted by the short-circuiting key. Finally, by opening the line switch, buzz and shock are eliminated, and the 'voluntary' reaction time to the click of the key may be measured.

The excursion of the signal-marker, together with that of a time-marker, is recorded on a smoked drum, spun rapidly by hand to obtain a higher rate of movement than can be secured by the usual kymograph motor. The time-marker vibrates at 120 cycles, being operated by the 110-v. 60-cycle A.C. current supplied by the lighting circuit. We obtain an excellent time-line by using an inexpensive marker supplied by the Cambridge Instrument Company, in series with a 100-w. bulb. The spring and set screw must be adjusted carefully, and then the marker will run quietly for hours without further attention. Such a marker should be checked occasionally against a fork, if one is to be certain that it is not vibrating at 240 vs. Once the proper adjustment is secured, the marker remains constant. This lighting-circuit time-marker seems to the writers to be superior to the tuning fork, because of its ease of manipulation, its quietness, and its accuracy. It permits readings to about 4σ.

In setting up the apparatus for the experiment, the markers are placed close together. This permits the recording of ten presentations on one drum. *S* is placed with his back to *E*. The experiment may be carried out as follows:

First, several presentations of the buzz alone are given, to show that it does not cause a withdrawal of the hand. Then the buzz and shock are presented simultaneously many times, until a fairly stable conditioned response is formed. One test is made some time during every group of ten presentations, by omitting the shock. Records are not taken continuously during the training period. After five conditioned responses have been recorded, experimental extinction is carried out. After this, the line switch is opened, and five 'voluntary' responses to the click of the key are recorded. A record taken by an undergraduate student is shown in the accompanying figure. Above each line of the signal-marker is the time-line (1/120 sec.). The downward excursion of the signal-marker indicates the presentation of the stimulus, and its return shows the moment at which *S* responds. The record shows unconditioned responses ('Unc. R.'), conditioned responses ('C. R.'), and several failures to respond to the conditioned stimulus ('No Resp.'). At the bottom the time-line (1/120) is compared with the tracing of a fork (1/100).

In the present arrangement, *E* must indicate on the record whether or not the shock has been short-circuited. It was thought best not to complicate the arrangement by the addition of another signal-marker to show this item automatically.

The writers feel that this experiment has a number of advantages. It gives each student an opportunity directly to experiment with the conditioned response. The recording is so simple that it can be done readily by an untrained *E*. Finally, it permits him to make quite accurate determinations of the latent time of simple reactions. By comparing the times he obtains here with those he gets later from 'simple' and 'choice' reaction experiments using the chronoscope, the student obtains a better understanding of the phenomena of response than is the case by the use of the chronoscope alone.

AN ELECTRICAL STIMULUS-APPARATUS

By Thos. H. Howells, University of Colorado

Psychological experimentation necessitates accuracy in control of the various stimulating factors in order that observed responses may carry significance. The advantages of electricity as a stimulating agent have long been recognized. Some of these advantages are (1) wide range of intensity, (2) accuracy in timing and counting, (3) ease of application to the various sensory areas, (4) quick and automatic application, (5) objective recording of data, (6) remote control, (7) freedom from harmful after-effects, and (8) relative absence of previous conditioning as regards the electrical stimulus.

The difficulties encountered in the past in using the electrical stimulus seem to be chiefly the following: (1) lack of dependability—variations in contact and in tissue resistance have made it impossible to predetermine and maintain a given intensity or, in other words, to gauge the 'dose' to the *S*; (2) lack of flexibility—difficulty has been encountered in attempting to vary the stimulating current in a uniform and certain way, and (3) lack of means of accurate measurement of the intensity of the stimulus. The common origin of these difficulties seems to be the nature of the current supply. In order to obtain the requisite high-potential, and yet avoid the danger of a severe shock inherent in a high-voltage transformer, it has been customary to use an induction coil with an interrupter in the primary circuit. The output of either the transformer or the induction coil is of alternating potential. Alternating-current meters of sufficient sensitivity and range for measuring the necessarily very weak current involved are not on the general market. It has been the practice, therefore, to substitute some means of measuring or estimating the EMF, or voltage across the points of contact.

It should be remembered, however, that EMF units are not valid measures of the stimulating energy, except when the resistance across the points of contact remains constant. This is seldom the case in physiological work because of the variability of skin resistance and firmness of contact. When different *S*s, some having moist skin and some very dry skin, are used the applied EMF is not even roughly proportional to the intensity of the shock. Most certainly voltage is not, as popularly supposed, the sole cause of the shock.

The effective stimulus energy is closely proportional to the square of the current (according to the formula $E = I^2R$), since resistance between points within the body are fairly constant. Current units would, for this reason, appear to be the better measures of the intensity of the stimulus.

In order to overcome the difficulties as outlined above, an apparatus was devised of which the following is a description. Alternating current at 10 v. potential was obtained from a 200-w. toy transformer (T1 in Fig. 4). This was passed through a control rheostat (R1) having a total resistance of 100 ohms and a capacity of 0.5 amperes. From thence the current was passed through the primary of a step-up transformer having a normal output of 50 milliamperes at 20,000 v. with a 110 v. input. This instrument was a one-kilowatt spark transformer of the kind used for radio spark transmission. Any high-voltage transformer can be used, or even an induction coil with the vibrator shorted, provided the latter has a high step-up value and good current capacity. In the latter case the voltage supplied to the primary and the value of the series resistance will have to be determined experimentally. It should be kept in mind that the general object of this system is to obtain, from a high-resistance source, a potential which can be readily varied over a wide range.

The output from the transformer, T2, is led through the electrodes, or body applicators, and from thence through a D.C. meter or galvanometer (range, about 5 milliamperes), and next to both the grid and plate terminals of an ordinary radio receiving tube of the 226 type. The filament of this tube can be heated by current drawn from the appropriate supply terminals (2.5 v.) of the toy transformer, T1. The vacuum-tube changes the alternating current to direct current, which is drawn from the filament connection and then flows back to the other terminal of the high-voltage secondary, thus completing the circuit.

It will be apparent that this system has the following advantages.

(1) The intensity of the stimulating current is readily and smoothly varied from an inappreciable amount up to the maximum for safety, through adjustment of the control rheostat, R1.

(2) The magnitude of the objective stimulus is registered at all times by the meter, which measures the actual current flowing through the tissues.

(3) The amount of current, or 'dose' can be very readily predetermined by adjusting the rheostat while the electrodes are shorted. The increased resistance of the tissues between the electrodes will not greatly reduce the current below this setting.

(4) Variations in contact, and moisture of skin do not have an appreciable effect on the quantity of current or the intensity of stimulation. This uniformity is due to the fact that the resistance of the body is only a small fraction of the total resistance of the circuit in which it is placed. The combined resistance of the secondary of T2 and of the vacuum tube amount to some 500,000 ohms, while the resistance between the thumb and mid-finger of the hand amounts to only about 30,000 ohms. This ratio can be increased by inserting more resistance anywhere in the circuit (*e.g.* at R2, 1 megohm). This will result in even greater current stability. The total resistance, outside of the body, should be sufficient so that variations in the moisture of the skin or of the pressure of the contacts against the skin will not cause an appreciable movement of the meter.

(5) There is no danger of a serious shock to an *S* with properly constructed apparatus, as the maximum current is limited to a small value by the high resistance of the external part of the circuit.

(6) The apparatus is noiseless in operation.

A PROJECTION TABLE FOR STUDYING MOTION FILM

By H. M. Halverson, Yale University

No motion picture projector in the market today adequately fulfills the requirements for the analysis of the motion film. At the Yale Psycho-Clinic we have constructed a special form of projection table (shown in Fig. 5) to facilitate the study of the motion film. The apparatus consists of an Eastman Model C Kodascope, equipped with a one-inch lens, mounted beneath a table, within easy reach of the left hand, in such a manner that with the aid of a mirror it projects the image of the motion film upward on a ground glass set in the lower left hand corner of the table top, which tilts forward 30°. The Kodascope which normally permits the film to move only forward in the film channel is altered, to permit film movement backward as well as forward, by the adjustment of the teeth of the shuttle and by the addition of two take-up pulleys at either end of the film.

The apparatus has the following advantages. (1) The film may be studied in forward or reverse motion. (2) The projector may be operated to give any desired speed of the motion film, rapid motion, normal motion, slow motion, discrete motion, and still framing. (3) Any frame of the motion film may be stilled and studied at length without fear of damage to the film. (4) By substituting clear glass for the ground glass an exact duplicate of the image may be traced on paper. (5) The ground glass may be lined to aid in defining directions and dimensions. (6) All parts of the apparatus and the projected image are within easy reach. (7) The location of the projector permits the left hand to operate it, so that the right hand may be free for recording. (8) The cost of the projector with its additions is remarkably low in comparison with other projectors which have some of the above advantages.

APPARATUS NOTES

A Floating Mirror Technique for Recording Eye-Movements

Essentials. The essentials of the floating mirror recorder of eye-movements are first a minute fragment of thin convex mirror 1 to 2 mm. square and of any convenient focal length, so embedded in paraffin as to fit the surface of the cornea without offering any unprotected edges to damage the tissues of the eye; and secondly, a recumbent subject with corresponding orientation of the recording light and camera and the object of regard. If necessary the lids may be artificially fixated for short periods.

Advantages. Some of the advantages of the floating mirror recorder are as follows: (1) It gives a precise record of successive positions of the point of regard as well as the time sequences and direction of eye-movmeent. (2) It is the first technique for directly recording the position of fixation practically uninfluenced by slight movements of the head. If a corresponding record of the head-movements is obtained by a secondary mirror the relations of head- and eye-movements may be studied with great precision. (3) Since records are produced by deflection of a beam of light by a concave mirror the resulting curves may be highly magnified. They may, however, be reduced to exact correspondence with the object of regard if the source of light is placed at a considerable distance and if the objects of regard are twice as far from the eye

as the recording camera. (4) For the accurate recording of movements in one meridian plane there need be no distracting beams of light in the field of vision if the source of light and the orientation of the camera and the floating mirror are properly adjusted. (5) There need be no distracting fixation of the head by the application of mechanical contrivances. The presence of the floating mirror is not felt on the cornea and is perceived only when the mirror touches the tarsal edges of the lids. (6) The floating mirror may be used with hospital patients who are too sick to leave their beds.

Preparation of the mirror. Concave mirrors of any desired focal length can be produced easily and cheaply by silvering corresponding glass wafers such as are used by opticians for making cemented bifocal glasses. A convenient strength of such plano-convex wafers is 1 diopter, having a principal reflecting focus of about 23 mm. These wafers after being silvered on the convex side may be cut with a diamond or crystal of carborundum to any required size. Those mirrors that come from the edge of the wafers are lightest, but even those that come from the center of a 1 diopter wafer are so thin as to produce no appreciable physiological disturbance.

After cutting they should be laid plane side down on a glass surface and embedded in a drop of melted paraffin. Before complete cooling has taken place this drop of paraffin should be impressed with a convex lens which has approximately a 7 1/2 mm. radius of curvature. This conforms the paraffin to the curvature of the cornea. The conforming lens should not be warm, otherwise the paraffin will stick to it. It should, however, be pressed down nearly to the back or silvered surface of the glass mirror so as to make the minute flake of paraffin as light as practicable. After being formed by the lens the edges of the paraffin flake should be trimmed away by a sharp knife (such as a razor blade) to an octagonal form so as to thereby reduce surface tension if the paraffin touches the lids.

Orientation of the floating mirror. The application of these embedded mirrors to the approximate position on the cornea may be made quite innocuous by the use of a fine badger hair brush moistened in normal saline solution. The final orientation of the mirror can usually be made by manipulation of the lower lid. Optimal positions for the concave mirror appear to be slightly to one side of and below the pupil, thus interfering to the least possible degree with vision.

The adherence of the mirror to a given position on the cornea depends on surface tension. This is ample for the recumbent subject only. The mirror will gradually descend if the subject stands or sits. In this respect our miniature glass mirrors differ from the flakes of Chinese white used by Judd.

The mirror is easily dislodged from the surface of the cornea by manipulating the lower lid so as to shift the flake of paraffin to the inner bay or angle from whence it may be recovered either by winking or by use of the brush.

Safety of the floating mirror. The floating mirror can do no damage to the surface of the eye because of the following reasons. (1) Its edges are entirely protected by a smooth coating of a physiologically inert substance which has a melting point considerably higher than body temperature. Even if, through winking or rolling the bulbus, the mirror gets under the eyelid there is no cause for alarm. The mirror-wafer is non-irritating and readily slips out from below the lid. (2) The mirror adheres to the cornea from surface tension

only which prevents any possibility of tearing the epithelium during winking and adjustment, or at removal. (3) Its mass is too little to deform the cornea to a noticeable degree. (4) Though no one knows how much weight the bulbus can carry without disturbing the behavior patterns of the eye, it is probable that the work involved in carrying the minute mass of the embedded mirror is much less than the normal work done in deforming the lids at every eye movement.

Orientation of the recording light beam. The greatest variety of arrangements is permissable in conforming the optical system to the various experimental situations. We conjecture that in general it is advisable to cut out the physiologically active light rays with a blue filter and that the fine adjustment of the recording pencil of light would be better secured by manipulating plain mirrors to change the angle of incidence rather than by attempting fine adjustments of the position of the floating mirror. It seems to us also inadvisable to suspend the recording camera directly above the patient's head. This can be obviated by redirecting the deflected beam with a plain mirror to a horizontal camera slit placed at the rear.

Institute of Human Relations — Raymond Dodge
Yale University — Walter R. Miles

A Simple Test for Ocular Dominance

The phenomenon of ocular dominance is receiving increased attention and various devices for testing it objectively are constantly suggested. The simplest and most satisfactory one, however, is usually not mentioned. This is the paper-hole test that requires no equipment besides a sheet of paper. Directions for conducting this test are as follows:

Tear a small hole (about 1/2 in. in diam.) in a sheet of paper. Place the bit of torn out paper upon the floor or table. Require the *S* to hold the paper with the hole in it at about 15 or 20 in. from his face and to look through the hole at the bit of paper on the floor or table. Without permitting him to move the sheet of paper or his head, require him to close his right eye—if he still sees the scrap of paper he is left-eyed; if he is no longer able to see it, he is right-eyed. The same procedure may then be followed except that the *S* be required to close his left eye in order to explain the principle to right-eyed *S*s or, in the case of right-eyed children, to add interest to the experiment.

This test is reliable. Its principles are those underlying complicated tests for eye dominance. It has been found to be entirely satisfactory in the case of children even in the first and second grades. (The experimenter must cover childrens' eyes for them.) It can be used for close range vision and also for vision as far as the eye can see, by locating a tower, tree, or object at a great distance. (Thus eye dominance for near and far vision can be compared.) Its accuracy is not decreased when the subject knows the purpose of the test for it is only by special and readily detectable adjustment that the subordinate eye is accommodated.

The only precaution that the experimenter must take is that the *S* does not bring the sheet of paper up close to his face as in the peep-hole or key-hole test for other factors will then enter in, such as the handedness of the subject, the handedness of the experimenter in handing the sheet of paper to the subject, irregularities in the shape or placement of hole in sheet of paper, that would suggest bringing the hole to one eye in preference to the other.

Los Angeles, California. — Norma V. Scheidemann

The Bidimensional Reproduction of the Surface of a Tridimensional Object

In connection with a study of nerve regeneration that Dr. Harry Helson and I have been engaged in since November 1924, we found it desirable, in order to indicate the progress of regeneration on the printed page, to reproduce bidimensionally the surface of the tridimensional object of experimentation—the skin of my left index finger.

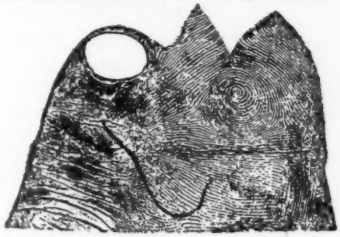

Fig. 6. Distal Surface of Left Index Finger

The area of insensitivity (resulting from a nerve section) extended from the cut over the dorsal, radial, and ventral sides of the finger to the tip. The finger-print method of reproducing the convolutions, *i.e.* inking and rolling the finger over suitable paper, was tried, but the finger tip, which was one of the crucial areas, could not by any variation of the method be reproduced in its proper relation to the rest of the finger. The finger-print method had, therefore, to be abandoned and other attempts made. The problem seemed simple enough: the reproduction of the surface of the finger in a medium that could be spread in a single plane and photographed. Various substances were tried—putty, gelatine, glue, celloidin, plaster of paris, rubber—but none yielded a perfectly plane surface. It was not until Dr. Wm. C. Geer's services were enlisted,[1] and various solutions of rubber and methods of procedure were tried that this technological problem was solved.

Of all the solutions used in Dr. Geer's laboratory the one that gave the best results was liquid rubber latex. The finger was carefully soaped, so that the rubber would not adhere directly to it, and then a thimble, or finger stall, was built up by alternately dipping the finger in the latex and drying it in a warm air-blast. When the stall had reached a thickness of approximately 2 mm., it was cut along the inner edge of the dorsal side and across the end of the finger just above the nail. The stall was then removed, a single cut was made in the concave area of the ventral side (the ball of the finger), and the stall was then spread out flat and glued (finger side up) with rubber cement to a plane surface. This was then turned over to the engraver,[2] who dusted it with white powder, photographed it, and made the cut shown in Fig. 6.

This method may readily be used whenever a bidimensional reproduction of the surface of a tridimensional object is desired.

Karl M. Dallenbach

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[1] I am indebted to Dr. Geer, formerly director of the research laboratory of the Goodrich Rubber Co., for his advice, assistance, and the use of his laboratory and materials.

[2] I am also indebted to Mr. H. J. Van Valkenburg of the Ithaca Engraving Company for the attention and care that he gave to the preparation of this line cut.

NOTES AND DISCUSSIONS

Brain Localization by Albertus Magnus and Some Earlier Writers

The belief that different psychic processes have their organs in different parts of the brain was held before the time of Albertus Magnus;[1] but the aim of the present paper will be confined to Albertus' views, with a word added as to the sources from which he drew. According to Albertus,[2] the brain, taken largely, is divided into its enveloping membranes, its medullary substance, and its ventricles; these last being filled with a vapor of especial use to the *anima* or *psyche*, a vapor called 'animal spirit.' The brain is in continual motion by reason of the motions of the motor nerves, very many of which arise in the posterior part of the brain, and is subject to continual effects from the motions of the sensory nerves forward, where are located the cerebral organs suited to receive the sensory forms.[3]

The ventricles are three:[4] the anterior—which we call the 'lateral' ventricles—large and divided into a right part and a left; the posterior—our fourth—smaller than the anterior; and the middle—our third—which is not so much a ventricle, speaking strictly, as a passage-way from the anterior to the posterior ventricle.

In these ventricles—often explicitly, and at all other times, I incline to think, implicitly and by intention—are located such of our psychic powers as have special organs. For, as we shall see later, there are psychic powers which are not 'localized,' as well as psychic powers which are 'localized.' Sometimes also, as I have suggested, the localization is not explicitly in a ventricle.

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[1] Boring in his admirable *History of Experimental Psychology*, after telling of the doctrine that the brain is the seat of the mind, speaks (p. 47) as follows: "Even the belief in particular localization is quite old. Albertus Magnus (1193-1280) referred judgment to the anterior portion of the brain and memory to the posterior, and placed imagination in an intermediate position."

The cerebral regions Albertus had in mind, and the psychic powers assigned to these regions, as understood by the present writer, will be found to differ in some important respects from those indicated by Boring.

[2] The sources of the present account are the following, to be found in *Alberti Magni Opera*, edited by Petrus Jammy and published in 1651.

De anima; in vol. 3; cited as *De an.*

De memoria et reminiscentia; in vol. 5; cited as *De m.r.*

De motibus animalium; in vol. 5; cited as *De m.a.*

De animalibus; in vol. 6; cited as *De a's.*

Compendium theologicae veritatis; in vol. 13; cited as *C.t.v.*

Summa de creaturis; in vol. 19; cited as *S.d.c.*

Philosophia pauperum sive isagoge in libros Aristotelis, etc.; in vol. 21; cited as *P.p.*

[3] *De a's.*, Bk. 1, Tract. 3. Cf. *C.t.v.*, Bk. 2, Ch. 36.

[4] Variously called: *e.g. cellae*, *De an.*, Bk. 2, Tract. 4, Ch. 7; *cellulae*, *S.d.c.*, Pt. 2, Q. 36, Art. 3; Q. 38, Art. 3; *C.t.v.*, Bk. 2, Chs. 38, 39; *ventres*, *De a's.*, Bk. 1, Tract. 3; cf. *S.d.c.*, Pt. 2, Q. 35, Art. 3; *ventriculi*, *De a's.*, Bk. 1, Tract. 3; *concavitates*, *S.d.c.*, Pt. 2, Q. 33, Art. 3; Q. 36, Art. 3; Q. 38, Art. 3.

The anterior ventricle, sometimes subdivided into an anterior, middle, and posterior portion,[5] assists in smell and in the excretion of certain substances from the brain, and receives the subtle vapor, the 'spirit,' which is of aid to various operations.[6] This ventricle is said to be filled, more particularly, with sensory vapor.[7] The vapor here has to do with special sensory power.[8] Here, besides, is located the 'common sense.'[9] Here also, but with less clearness or uniformity, are located certain functions connected with imagery and imagination;[10] and here is the power of appraisal;[11] but of these more will be said later.

The middle ventricle is at times thought to be the seat of the imaginative or 'cogitative' or 'formative' power.[12] The seat of 'fantasy,'[13] in spite of what has been said just above, is placed at the center, or thereabouts, of this middle ventricle, between the mnemonic and the imaginative power.[14] The middle ventricle, and particularly its anterior portion, is at times thought to be the organ of the power of appraisal,[15] although with a slightly different term[16] this power is assigned also to the 'first' part of the brain.[17]

In the posterior ventricle are "the vapors concerned with the preservative function; that is to say, with remembering and recollecting and many other things of this sort which maintain their activities by means of the vapor there."[18] Or again, and without the suggestion that a variety of cognate powers are involved, what is called simply the mnemonic power[19] is placed in the posterior cavity of the brain.[20] In the rearmost portion of this ventricle is the power of movement;[21] for in the posterior ventricle there is a motor vapor,[22] corresponding to the sensory vapor already mentioned as filling the anterior ventricle.

Albertus is thus fairly clear with regard to certain powers and certain places; and fairly unclear, it would seem, with regard to others. The special senses and the 'common sense,' if located in a ventricle at all, are always—so far as I have discovered—located in the anterior ventricle. Memory, and the

[5] *S.d.c.*, Pt. 3, Q. 35, Art. 3; cf. Q. 36, Art. 3.

[6] *De a's.*, Bk. 1, Tract. 3.

[7] *S.d.c.*, Pt, 2, Q. 33, Art. 3; *plena est spiritu sensibili.*

[8] *S.d.c.*, Pt. 2, Q. 36, Art. 3; cf. *De a's.*, Bk. 1, Tract. 3.

[9] *Sensus communis*: *P.p.*, Pt. 5, Ch. 14; *S.d.c.*, Pt. 2, Q. 33, Art. 3; Q. 36, Art. 3; *P.p.*, Pt. 5, Ch. 14.

[10] Here is located *imaginatio*, *S.d.c.*, Pt. 2, Q. 35, Art. 3; *P.p.* Pt. 5, Ch. 15; *imaginatio* and *phantasia*, *S.d.c.*, Pt. 2, Q. 36, Art. 3; *phantasia*, *S.d.c.*, Pt. 2, Q. 36, Art. 3.

[11] *Aestimatio*, *S.d.c.*, Pt. 2, Q. 35, Art. 3; Q. 36, Art. 3.

[12] *Virtus quae vocatur imaginativa, sive cogitativa, sive formativa*: *P.p.* Pt. 5, Ch. 16.

[13] *Phantasia.*

[14] *C.t.v.*, Bk. 2, Ch. 39.

[15] *Vis aestimativa*: *C.t.v.*, Bk. 2, Ch. 38; *virtus aestimativa*, *P.p.* Pt. 5, Ch. 18.

[16] *Aestimatio.*

[17] *S.d.c.*, Pt. 2, Q. 35, Art. 3; cf. Q. 36, Art. 3.

[18] "*In postremo [ventriculo] sunt spiritus servativae operationis, quae est rememorari & reminisci, & alia multa hujusmodi quae suas custodiunt per spiritum qui ibi est operationes*"; *De a's.*, Bk. 1, Tract. 3.

[19] *Memorativa virtus.*

[20] *P.p.*, Pt. 5, Ch. 19.

[21] *Virtus motiva*, *S.d.c.*, Pt. 2, Q. 38, Art. 3; cf. *De.m.a.*, Tract. 2, Ch. 1.

[22] *Spiritus motivus.*

motor power, if located in a ventricle at all, are—so far as I have discovered—invariably located in the posterior ventricle. But with respect to certain phases of imagination, and with respect to the power of appraisal, his opinion varies: these processes are given their place, now in some part of the anterior ventricle, and now in some part of the middle ventricle. This is the less surprising, however, since in ancient times—certainly with Galen—there was some readiness to regard the middle ventricle as, after all, but a portion of the anterior ventricle itself.

It ought to be noted, moreover, that Albertus, in giving his cerebral locations, does not always use terms which unmistakably point to the ventricles, or to some particular ventricle. He occasionally refers some psychic function to the anterior "part" of the brain, or to the posterior "part" of the brain.[23] I incline to think, but am far from sure, that in these cases he means a ventricle, since he is so explicit elsewhere; often, indeed, in the very same discussion he shifts from the vague term 'part' to some word that unambiguously denotes a ventricle.[24] Since the localized psychic functions, with him, are so intimately connected with the vapor or vapors filling the ventricles, it seems hardly probable that he would, on some other occasion, without warning or assigned reason locate a psychic power in some cerebral region that would require so profound a modification of his theory.

As to unlocalized and, for him, unlocalizable psychic activities Albertus is certain that intellectual power[25] is not confined to any particular region of the body, but is in the body entire;[26] neither intellect nor reason[27] operate in a special bodily organ.[28]

In closing may I note some of the sources from which Albertus himself drew.[29] He cites in this connection the Arabic writers, Averroës, Algazel, and Avicenna, mentioning the last of these with especial frequency. But he does not confine himself to the Arabians. Among others whom he names as giving

[23] *E.g.* in *C.t.v.*, Bk. 2, Ch. 36, the *sensus communis* is placed "in the anterior part of the brain, in that region where the sensory nerves of the five senses come together"; in *C.t.v.*, Bk. 2, Ch. 40, *memoria* is placed "in the posterior part of the brain"; in *S.d.c.*, Pt. 2, Q. 38, Art. 3, *memoria* is placed "in the posterior part of the head"; in *C.t.v.* Bk. 2, Ch. 37, *vis aestimativa* is located in the anterior or interior part of the brain.

[24] *E.g.* in *S.d.c.*, Pt. 2, Q. 35, Art. 3; Q. 36, Art. 3; Q. 38, Art. 3.

[25] *Virtus intellectiva.* [26] *P.p.*, Pt. 5, Ch. 25. [27] *Intellectus*; *ratio.*

[28] *S.d.c.*, Pt. 2, Q. 36, Art. 3.

[29] The present account and Boring's differ in the following. What Boring designates as 'portions' of the brain, here become at times definitely and unmistakably *ventricles* of the brain. Judgment, instead of being localized unqualifiedly in the anterior portion of the brain, is localized there only in so far as it is involved in the *sensus communis*, *imaginatio*, *phantasia*, and *aestimatio*; and in all these cases, except the *sensus communis*, it is localized also in the middle portion or ventricle; while such judgment as is involved in the *virtus intellectiva*, *intellectus*, or *ratio* is, for Albertus, not localized at all. Furthermore, Albertus gives a particular cerebral place to several important powers not mentioned by Boring; namely, the central motor activity, the central power of special sense, the *sensus communis*, and the *vis aestimativa*, or *aestimatio*. It may well be that Boring has sources which warrant his presentation. I should hesitate to suggest that I have found all the pertinent passages in Albertus' writing. In twenty-one folio volumes one may easily overlook what would justify an important change in the present account.

particular cerebral localizations, are Apulius, Constabulus, Gregorius, Damascenus, and Galen. Evidently there was available in his day a considerable body of authority for the localization of psychic functions in particular parts of the brain. One thus is invited to trace the history of the matter to a time far earlier than the thirteenth century. Their own writings that have come down to us give assurance that Avicenna (980-1037) and Galen (131-200 A.D.) have a rightful and important place in the growth of this particular branch of physiological psychology.

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A CRITIQUE OF INSIGHT IN KÖHLER'S *GESTALT PSYCHOLOGY*

For a long time psychologists of the Gestalt school have been urging that we discount what they take for the efforts of analytic psychology to give a description of immediate experience. While they do not, to be sure, all discard analysis, they advocate that we adopt in preference to former efforts at least the pre-scientific view of consciousness which the plain man is supposed to hold. Thus, Koffka, in 1924, told us that the "larger wholes" are not open to analytical observation. "If an observer is to grasp their nature he must adopt a very different attitude, such as the attitude of . . . the wise old doctor, . . (or) . . the clever diplomatist or the intuitive poet."[1] Now Köhler explicitly writes that for a more adequate account of conscious experience we must go to the layman. "In the treatment of sensory process Gestalt psychology prefers the data of naïve description to the findings of prejudiced introspection."[2] By the findings of prejudiced introspection Köhler means "a sensory field regarded as a mosaic of indifferent spots,"[3] and it is this conception which he attacks throughout his discussion of insight, and in preference to which he chooses what he conceives to be the layman's intuition. He illustrates the position of the psychologist who is interested in giving an introspective account of experience in this manner: "I hold my arm horizontally for a while. Very soon the arm will not remain in that position unless I exert a special effort. In my total field there is, besides this effort, the blue sky, a lark's song, the aspect of the lifted arm, a smell of moist ground. There is also a curious feeling in the lifted arm, growing more intense as the moments go by, and, with this feeling, something like a downward pull in the arm." He goes on to comment, "If someone tries to describe the situation in terms of indifferent data, each of which has a place, . . . and all of the data having hundreds of mutual 'relations' as to place, distance, succession or simultaneity, intensity, and so on—he will not even touch what psychologically is the main feature of the situation."[4] Again, "We cannot experience enjoyment as something apart, among a pattern of hundreds of other things."[5] Everywhere is the implication that the "analytic psychologist" views experience as a mosaic of discrete states, in which he infers connections between certain components as a result of a rational process, based on numerous past experiences in which these components have occurred together.[6]

[1] K. Koffka, Introspection and the method of psychology, *Brit. J. Psychol.*, 15, 1924, 161.
[2] W. Köhler, *Gestalt Psychology*, 1929, 350. [3] *Ibid.*, 351. [4] *Ibid.*, 358 f.
[5] *Ibid.*, 376. [6] *Ibid.*, 353.

Köhler gives no references to psychological studies in which one may find such accounts as the above. His relevant documentation is limited to quoting and paraphrasing of Hume, in which he neglects the important fact that Hume's interest was fundamentally epistemological. In the view of all investigators who have controlled the conditions of introspection, it would be impossible for the field of clear experience to contain on a par at the same time the effort, the blue sky, the lark's song, the aspect of the lifted arm, the smell of the moist ground. Most of these would be non-present or subordinately present for the observer in any specific situation, as a matter of the usually-called limitation and pattern of attention, instead of being the mosaic of indifferent experiences which Köhler attributes to the introspective analysis. Consequently no psychological sense attaches to the assertion that "geometrically, as it were, or logically my *actual experiences* allow a large number of different combinations into pairs. My effort may be combined with the lark's song, the smell, the color of the lifted hand, the blue of the sky."[7] No wonder Köhler "chooses the layman's belief whole-heartedly"![8]

For Köhler, experience, unspoiled by prejudiced introspection, apparently consists in an indefinite number of events, among which certain ones are felt or known to be the natural outcomes of others, and to be unrelated to still others.[9] The thing upon which he insists is that in the total field of experience we have not only the *results* of organization, present as segregated wholes, but also the 'whys' and 'hows' of organization; to these latter entities he applies the term 'insight.' "A total field would be experienced *without* insight, if all its several states, wholes, attitudes, *etc.*, were simply given as a pattern, in which none was felt directly to depend upon any other and none to determine any other."[10] Köhler has many ways of referring to insight, some of which seem more descriptive of actual experience than others. Thus, he writes "fright is *experienced as* jumping at us right out of the very nature of certain events."[11] This figure seems more applicable to the experienced phenomena than do such expressions as the following: "I feel myself *necessarily growing 'nervous' under the pressure of* sticky air and loud, disorderly noise,"[12] or my thought of a drink, which tends to appear when I am thirsty, is "felt to be the natural basis of longing *for* it."[13] In the above, and indeed in the great majority of his references to insight, Köhler uses expressions which seem to the writer to be indicative of after thoughts, or reflections, about experience, and not to be descriptive of the ordinary working mind. Thus, for instance, Köhler maintains that the layman understands directly why he is inclined to do one thing in a certain particular situation and a definitely different thing under subsequent different conditions.[14] Again, an attitude *is experienced as depending definitely upon something definite*[15] or is "felt as being founded on the properties of" an event.[16] Enjoyment is felt as the adequate attitude *belonging to* those actual experiences, or as their natural result. A "craving is experienced as being determined directly by just that sort of 'idea'."[17] The layman "takes it as the simplest matter in the world, that he has a feeling now 'because of' a

[7] *Ibid.*, 358. Italics mine. [8] Ibid., 350. [9] *Ibid.*, 368. [10] *Ibid.*, 372.
[11] *Ibid.*, 356. [12] *Ibid.*, 356. [13] *Ibid.*, 366. [14] *Ibid.*, 349. [15] *Ibid.*, 353.
[16] *Ibid.*, 353. [17] *Ibid.*, 366.

certain aspect of his actual situation, and that he feels the 'because,' too. We experience '*why*,' issuing from just such an event, thing, or attitude, a given effect should be just the one we find growing out of it."[18] "We feel *how* . . . . one tendency grows directly out of the nature of one situation."[19] We feel an actual "reference to," not merely a "coming together" or a "following each other."[20]

The present writer objects to Köhler's "layman's consciousness" for the reason that naïve experience—by which can only be meant the mind in action other than reflecting upon or describing its past action—does not bristle with "insights" of the above described sort. These latter are rather reflective turns or verbal responses elicited by social conscious situations—need of communication of the purport of experiences; or they are brought in as after-thoughts in an evaluative or interpretative set. Thus, if the layman were asked whether he knew why he made a certain response—why, for example, he moved away from a hot radiator—he would undoubtedly say that he did; but it would not follow that such a reflection was present in connection with the actual moving away.

In support of this objection—that Köhler has not really described naïve experience—the writer wishes to refer to some evidence obtained in the course of certain experiments reported in 1916.[21] The experiments in question were essentially efforts to produce in the laboratory conditions under which an observer might form a concept from relatively novel material, and to study the evolution of the concepts so formed. The method consisted in presenting a series of nonsense figures which showed wide variation in many respects, but contained also certain recurring common features. A nonsense name was given to the group. The *O* looked at the figures one by one for a brief period, formulated a definition, and at intervals reported the perceptions and other processes present during his observation, and instrumental in forming his definition of the group concept. In later experiments he was shown still other figures,[22] which might or might not contain the repeating and therefore essential features. The instruction here was to determine in the case of each figure whether it belonged to the group or not.

For the most part, especially at the start, the *O*s were not aware of the actual goal of the experiments; they followed the instructions to define and to classify, and they then gave minute accounts of the rush and lag of experience which in their cases constituted the observing and classifying of the figures, under the experimental instructions. They were urged to report everything that occurred, in any terms they wished, but to attempt to give an existential account of the meanings which appeared, in addition to communicating their existence. Certainly, in their preoccupation with this experimental task, they did not report mosaics of sensation. In no sense were they impelled by any felt need to find conscious processes matching sensory stimuli (including muscular

[18] *Ibid.*, 367. [19] *Ibid.*, 381. [20] *Ibid.*, 373.

[21] S. C. Fisher, The process of generalizing abstractions; and its product, the general concept, *Psychol. Monog.*, 21, 1916. The above discussion is based on pp. 97 ff., and 172-180.

[22] S. C. Fisher, An analysis of a phase of the process of classifying, this JOURNAL, 28, 1917, 57-116; esp. 97 ff.

postures) which were present. Their concern was to give, in so far as memory permitted, an existential description of their minds in the action of observing the figures under the determination imposed by the specific experimental situation. In other words, they were "laymen" in all essentials during the activity demanded by the task, but they differed from laymen in their subsequent responses to this experience. Unlike "laymen," they were free from the ordinary disposition to refer to their mental processes, if at all, in terms of the explanations which alone are intelligible in the usual social situations. They were absolved from the common social necessity of making the practical import of their experiences clear; and furthermore they discriminated between existential experience and its evaluation or interpretation.

Now it would seem that the "layman," as drawn by Köhler, would have been aware, in observing the oncoming figures, that he was looking at certain regions because he had formerly found recurring similar and therefore significant features there. Again, in defining the nonsense word which had been affixed to the group, he would have reported that he specified such and such a feature because he knew it to be general—that is, because he was aware that it had occurred in all members of the series. These would have been the "insights" "natural" in the particular situations in question. Yet the *O* bent on reporting the continuum of experience incidental to his perceiving and defining the groups, usually mentioned no such "insights" or meanings. The feature simply behaved as a general one, without awareness that it was general. When *O* did report an awareness that the feature was general and must be included, he often described this experience in terms which were precisely the same as those elsewhere used to express merely the manner of perceiving the features—*i.e.* the behavior in consciousness of the features which had proved to be general. Moreover, the affixing of a meaning label was relatively rare, occurring only in about 11% of the cases.

Now, as has been emphasized, the instructions by no means forbade the report of "meanings," "awareness of what one was about," or the like. On the contrary, the *O*s were asked to report these when they occurred, and also if possible to describe their process aspects, or experienced character. The infrequency with which meanings were reported as having occurred during the operation of the vigorous "determining tendency" set into play by the experimental conditions consequently impressed the writer, who made a special effort to determine the general conditions under which the *O*s affixed to their experiences certain of the meaning labels most to be expected.

The study revealed that of the experiences of generality or non-generality of a feature which were labelled as such, about 35% were complicated by the presence of more or less intensive kinaesthetic and affective processes, or by special imagery of one sort or another. These components as they occurred in their setting may have been the essence of the experience, or they may have been more prominent in recall and so favored its being labelled. Later experiences, in which observing and defining had become mechanized or routine, were never given an "insight" or "meaning" label. The remainder of the labelled experiences had no elements of particular prominence, nor any components which distinguished them in any way from the non-labelled ones. They were found exclusively in the reports of two of the *O*s. It appears then that the tendency

to affix a meaning label was to some extent an individual matter. The *O* who most frequently labelled his experience as a consciousness that certain features were general and so should be included in his definition had shown increasing concern with the object of the experiment, and had a growing interest in, or inclination to point out, anything that might be interpreted as an experience of generality. On a number of occasions, this *O* had commented to the writer upon differences between generalization and abstraction which he had found in his reading. His "meaning" or "insight" labels were apparently after-thoughts, given during his report in order to emphasize experiences which he thought would interest the experimenter. It is obvious that a descriptive psychology based upon meaning reports from *O*s who failed to distinguish between description of processes and their socialized interpretation would be of little value.

Undoubtedly any of the *O*s, if asked, would have said that he knew that the features mentioned were generally present in the figures. Undoubtedly also if asked for reasons for his defining in such and such a manner, he would have stated that he did so because the features mentioned were invariably present in the members of the group. But the important thing is, that neither reply would have signified an actual feature of the observing or defining continuum. It would rather have denoted an after-thought, a verbal response made to a situation which demanded that the *O* rationalize his process so that another person could understand its bearing upon a matter in hand.

It appears to the writer, then, that Köhler has mistaken for parts of the dynamic, non-communicated conscious stream, entities which are frequently modes of speaking in response to the social need of communication, the need of making one's experience appear sensible to an auditor or reader. Only in connection with the rather unusual demands made under certain experimental conditions would an *O* give a straight-forward retrospective account of his perceptual experiences, without feeling the social need of switching these into terms readily understood for practical conversational purposes. Another circumstance greatly favoring the *ex post facto* injection of "insights" into the supposed naïve continuum of experience would be a theoretical interest, such as was present in one of our *O*s—and in his discussion of the nature of "insight" this is obviously present for Köhler. The "insights" have systematic work to do. They are needed in connection with the dynamic aspects of the physical Gestalt. As such they are not without reminders of the "innervation sense" of bygone days, and of other conscious components assumed in response to some theory of neural or motor activity.

"Insight," then, is a questionable aid in any back-to-the-obvious movement. Köhler's layman is a bit "sicklied o'er with the pale cast of thought," perhaps a literary fellow, and probably somewhat bothered by inability to get many of his lesser activities nicely mechanized. "Insight," as a means of holding the states, wholes, attitudes of experience together seems little preferable to the "inferred connections" of the "analytic" psychologist. The writer fully agrees with Köhler in his stress upon the dynamic character of experience, if by this one means that it is enormously important not to neglect the temporal adverbs and the accounts of experienced contexts and attention (clearness) relations in observers' accounts of their activities. This is why it seems more accurate to say that fright leaps at one out of the very nature of a certain ex-

perience, if this may mean that an absorbing and intensely vivid experience is instantly shot through with the tumultuous organic and kinaesthetic components of the fright attitude; but this absorption, with the complete or well-nigh complete absence of other elements, would be the essence of the connection. The vivid togetherness of the experiences is the thing, and not a specific awareness that the experience caused or determined the fright. The writer sees no *a priori* objection to "insight" as a working concept in animal research, provided one puts no descriptive burden upon it. But the nature and frequency of "insight" and the conditions under which it occurs are problems. Certainly the assumption that "insight" is a characteristic and very common feature of unreflective human experience cannot be made without more ado on any evidence at present available.

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DR. HELSON'S 'CIGARETTE ILLUSION'

The 'Cigarette Illusion,' reported by Helson,[1] seems to lose its 'illusory' character with further study. Experiments, which have been carried out at Bryn Mawr College and at Cornell University, show unequivocally, I believe, that the 'illusion' in question is nothing but a contrast phenomenon.

The experiments were performed in a dark-room. The source of illumination used at Bryn Mawr was a frosted incandescent bulb, (60 w.), covered with gelatine filters of different colors, (yellow, amber, green, blue, red). At Cornell, we used special colored bulbs (60 w.) manufactured by the Western Electric Co. The objects used to produce a glow were: (1) cigars and cigarettes,[2] and (2) small pocket flashlight bulbs, painted with art enamel and then covered with white tissue paper so as to produce a diffused light approximating that of the cigarette.

Our findings substantiate Helson's in many respects. The usual reddish-orange of the cigar or cigarette tip is seen as yellowish green or green under red illumination; as more reddish than usual under green illumination. Our results differ from Helson's in that, under blue illumination, the glowing tip is noticeably yellowed, whereas, under yellow, it takes on a violet or purplish tinge for some, but not all, observers. The standard used for comparison was the color of the glow as seen under a Palo-Daylight lamp in a dark-room.

Like Helson, we find that the change is more marked under red illumination than under other colors. This difference may be adequately explained in terms of the physical composition of the lights. It is not due to any peculiar effect of red light *per se*. In the first place, spectroscopic analysis of the cigars and cigarettes showed green to be the predominant component, extending over approximately half of the visible spectrum. The remainder consisted of a relatively broad band of red and a narrow band shading from greenish yellow to orange. No blue was visible.[3] It might, therefore, be predicted upon a purely physical basis that any condition which tends to mask the red component

[1] H. Helson, A new visual phenomenon—the cigarette illusion, *Psychol. Rev.*, 37, 1930, 273-275.

[2] The brands used were Garcia Grande cigars and Lucky Strike cigarettes.

[3] These findings disagree with Helson's (*op. cit.*, 274).

would change the color of the glowing tip to green, while the masking of other colors would produce less marked color changes. In the second place, the illuminating lights were not of equal physical purity. The red lamps showed only a red band in the spectroscope, with perhaps a just noticeable shading toward orange—a relatively pure red light; whereas all the other lamps showed a full complement of spectral colors from red to blue, differing only in the proportions of the several components. Since these lights are not physically pure, we should not expect them to yield as good contrast effects as the pure red light. To test this hypothesis, we compared the effect under the pure red light with the effect under a red light which showed considerable green, yellow, and orange in the spectroscope. The greening of the glowing tip was noticeably diminished under the latter light. The color was a yellowish green, not a pure green as it was under a pure red light.

The same effects were obtained when small flashlight bulbs, painted reddish-orange and covered with white tissue paper, were substituted for the glowing tobacco. This shows conclusively that the color changes are not due to any peculiarity of the tobacco itself.

Since, in every case, the contrasting color tends to appear, more or less obviously—and the cases in which the change is slight or questionable may be explained as due to the impurity of the light—it would seem that this might be regarded as a contrast phenomenon. In order to test this hypothesis, we carried out further experiments.

Before proceeding to an account of these experiments, let us consider Helson's reasons for rejecting the contrast theory. His first objection, that the green should appear best in the shaded portions of the ash rather than where light is actually being emitted, if the phenomenon is a matter of contrast, must be thrown out for the simple reason that the ash acts as a reflector, reflecting the red light and so appearing red,[4] whereas the portion emitting light does not reflect the red. We should, therefore, expect the contrast effect to appear only in the luminous portion, as is actually the case. Helson's second objection must be met by experimenting, such as we have done. The third objection, that we do not see as striking a yellow under blue illumination as we do green under red illumination is met and explained by the spectroscopic analysis of the lights (*vide supra*).

Helson's second objection, if valid, would certainly necessitate the abandonment of the contrast theory. He says, "When no background of red is allowed to reach the eye, the end of the cigarette alone being visible, we still get the green effect." The validity of this objection is a direct function of the method used to eliminate the red background. Helson's method—the method which he used in demonstrating the "illusion" to students and colleagues—was to look at the glowing tip through a pin-hole in a white card. Does this eliminate the red background? Most certainly not. To be sure, the card shields the eyes from the direct rays of the red light, but the card itself is an excellent reflector, thereby furnishing a good red background for the glowing tip. Furthermore, the white wrapping of the cigarette—and to a less extent the brown wrapping of the cigar—reflect the red light. The conditions for contrast are almost perfect. We conclude therefore, that the contrast theory need not yet be abandoned.

[4] This is analogous to the case of gray color mixtures.

In order to eliminate the red background absolutely and so remove the conditions for contrast effects, we used a black body, consisting of a box 12 in. deep, by 10 × 10 in., lined with black velvet and covered with similar material. In the front side of the box, there was a hole 3.5 in. in diameter.[5] The red lamp was placed about 10 in. in front of the hole, (10 in. from the front of the shade-reflector), and about 20 in. to the right, so that none of its rays fell directly upon the side of the box opposite the hole. A Palo-Daylight lamp shade-reflector served to shield the *O*'s eyes from the direct red rays.

The cigarette (or cigar) was wrapped in dull-finished black paper, and the experimenter wore a glove of dull-finish black cloth on the hand which held the cigarette. The observer sat in front of the black box, with his eye about 20 in. from the hole. The cigarette was held immediately in front of the hole in the box during observations.

Under these conditions—rigid elimination of reflected red light in the vicinity of the glowing tip—the usual change in hue does not occur. All *O*s agree that the tip is either the same orange-red that it is under artificial daylight illumination, or else it is slightly more reddish. Red adaptation plays no noticeable part in the effect. The color of the glowing tip is the same at the end of a 10-min. period of adaptation as it is at the beginning.

To obtain a standard of comparison, white and gray screens were substituted for the 'black body,' the other conditions of the experiment remaining exactly the same. Against a white screen, the glowing tip is seen as yellowish-green, with yellow tending to predominate, while, against a gray screen of approximately the same brightness as the glow itself, it appears as an excellent green, or green with a very slight yellow resemblance. The best green was obtained with a screen of Hering gray No. 15.

The *O*'s position in respect to the source of illumination has some effect upon the phenomenon. When the red lamp is placed directly in front of the *O* at a distance of 2 feet, and the *O*'s eyes are shielded only from the glowing filament, so that intensive direct red rays are falling into the eye, the effect is better than when the glowing tip is viewed by reflected light only, as in the set-up previously described.

Another factor which influences the phenomenon is the difference in the intensities of the two light sources—the source of illumination and the source which shows the contrast effect. In general, one may say that the greater the difference in intensity, the better is the effect; but this may hold true only for relatively small differences such as we have worked with. Thus, we get a better green with a light of 60 w. than with one of 25 w. Under a given intensity of illumination, a cigar tip shows a better green than that of a cigarette,[6] the latter being relatively brighter. Furthermore, if a red lamp of 60 w. and one of 25 w. are placed 2 or 3 feet apart and lighted simultaneously, the smaller one will show a marked fading of the red, its color becoming orange or yellow-orange. In this case, the difference in intensity is not enough to produce the contrast effect, although there is a change in that direction.

[5] The author is indebted to Dr. F. K. Richtmyer, Professor of Physics at Cornell University, for suggesting this experiment.

[6] This fact has also been reported by Helson (*op. cit.*, 274).

To summarize briefly, we have shown: (1) that the 'cigarette illusion' described by Helson is not due to any peculiarity of the tobacco itself; (2) that the effect disappears when the conditions for contrast are eliminated; (3) that the effect is best when the background is of approximately the same brightness as that of the glowing tip; (4) that the effect diminishes when direct light is shut out from the eye; and (5) that the difference in intensity of the two sources is an important condition.

We conclude, therefore, that the 'cigarette illusion' is merely an instance of color contrast, not illusory—unless all contrast phenomena are to be considered *I*llusory.

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EXPERIMENTS ON SUSCEPTIBILITY TO DRUGS

Differential and abnormal psychology are both interested in those individual differences which involve varying susceptibility to the influence of stimuli or agencies introduced into or imposed upon the organism. Among these are differences in susceptibility to various drugs. The present paper presents certain data relating to susceptibility to the influence of caffein, with special reference to normal instability and to introversion-extraversion.

Native instability and susceptibility to caffeine. In earlier studies by the author concerning the nature of individual differences in susceptibility to the effects of drugs, several definite correlations have been reported. In the first place it was found, in the case of both alcohol and caffeine, that the most susceptible subjects were those whose normal performance in the tests used was inferior, and who showed least ability to improve through practice. General ability and learning capacity were negatively correlated with susceptibility to the drug.[1]

A second study reported the relation between susceptibility to alcohol and two measures of individual irregularity in normal performance, called respectively Stability Index and Coefficient of Variability.[2] Stability Indices were based on 21 hours of work, the work of the first half-hour and that of the second half hour of each period giving two series for correlation, much as the "odd and even" technique affords a reliability coefficient. The Coefficients of Variability represented the percent deviations of 42 separate trials from their own median. In both cases these measures were taken while the individual was working under normal or control conditions, and correlated with the effects of the drug on the occasions when that was administered. In the present report a similar study is made of the relation between Variability Coefficients and susceptibility to caffeine, based on an earlier report of a study of that drug.[3]

Data were chosen from three tests (Tapping, Addition, and Color Naming) which showed definite and similar effects in the original experiment (facilitation). Ten *S*s, who had in the 'intensive' section of the experiment received

[1]H. L. Hollingworth, The influence of alcohol, *J. Abn. and Soc. Psychol.* 18, 1923-24, 214-237; 311-333.

[2]H. L. Hollingworth, Correlations of achievement within an individual, *J. Exper. Psychol.*, 8, 1925, 190-208.

[3]H. L. Hollingworth, The influence of caffein on efficiency, *Arch. Psychol.*, 1913, (no. 22) 1-166.

the same dosage, after prolonged prior practice, were used. The measure of susceptibility was the difference between the performance on control days and drug days (stated as percent of the control record). The measure of normal variability was the average deviation of the trials of each individual on ten tests on two control days (stated as percent of the average). We thus secure for each individual two figures, for each of the three tests. One figure shows the relative susceptibility to the drug. The other shows the normal irregularity of the individual's work on control days. Since the cases are too few for the correlation method to be used, we can only study the individual cases, grouped according to these two measures into sub-groups. The three most susceptible individuals in each test, and the three least susceptible, constitute the two extreme classes. The remaining four individuals form a transition group.

That individual was called most susceptible whose change of performance, under the drug administration, was the greatest, and was in the direction of the net effect of the drug as shown by the average of all the cases. Smaller effects in this direction, and opposite effects, thus served to establish the individual's place in each test separately. In Table I we give simply, for each of the three groups, the average measure for each trait.[4]

Although we have divided the cases into three groups, there are seen to be as a matter of fact but two degrees of susceptibility represented. The top group of three cases show high susceptibility, averaging about 12% for the three tests. The remaining cases are very much alike, and should perhaps better be treated as a single resistant group, composed of 7 *Ss*, to be contrasted with the 3 susceptible.

The measures of individual variability show just the same situation. The average figures are always higher for the group that is really susceptible. The figures for the remaining two groups are very much alike; consequently we may consider them together, or use only the two extreme groups for comparison. The susceptible group shows measures of normal variability always greater, the average figures being 5.52 and 4.11, a difference of 1.41 points. Comparatively speaking, the normal variability of the susceptible group is about one-third greater than that of the resistant group.

In the case of alcohol the conclusion was drawn that "superior average performance, high stability index, low variability coefficients, capacity for gain through repetition, and high drug resistance, are positively associated, as are their various corresponding degrees." We may now conclude that a similar positive correlation holds in the case of caffeine, so far at least as we may trust results from this group of 10 *Ss*.

Introversion and caffeine susceptibility. When, the writer, some seventeen years ago, reported the results of experiments on the influence of caffeine on human efficiency, the terms extraversion and introversion had little vogue. People were then either romanticists or realists, reflective or executive, and tender or tough minded. The *Ss* of those experiments were not, moreover, classified in those terms. The recent suggestion of McDougall that extraverts

[4]Detailed results, which were computed by Miss Marjorie Elton, are on file in her M.A. thesis in the archives of the Department of Psychology, Columbia University, and in the University Library.

TABLE I

SUSCEPTIBILITY TO CAFFEINE AS MEASURED BY VARIATION IN TEST SCORES

Test	Degree of Susceptibility	Average Percent Susceptibility	Average Normal Variability
Tapping	Susceptible	7.96	4.85
	Transition	0.50	3.49
	Resistant	−4.00	3.48
Adding	Susceptible	9.00	4.92
	Transition	1.82	4.28
	Resistant	−0.80	3.98
Color Naming	Susceptible	18.83	6.78
	Transtition	5.45	4.46
	Resistant	−1.10	4.87
Average	Susceptible	11.93	5.52
	Transition	2.92	4.08
	Resistant	−1.97	4.11

and introverts show different susceptibilities to different classes of drugs led therefore to the following review of certain of the data of that early investigation.

Three of the chief experimenters, who during the six weeks of the caffeine investigation had become pretty well acquainted with the *Ss*, recently rated them on an introversion questionary. The inventory contained 25 points, selected as characterising the introvert, drawn from the lists of Jung, McDougall, Nicoll, Kempf, Allport, Wells and Freyd. A copy of this inventory is included in this report.

Five alternative ratings were provided on each question: Emphatically Yes, Yes, Doubtful, No, Emphatically No. The extreme positive and negative degrees were given a value of 2, the simple "Yes" or "No" a value of 1, and the "Doubtful judgments" were scored 0.

A net introversion-extraversion score could thus be secured, this being the algebraic sum of the 25 ratings for each individual. On the basis of these net scores the *Ss* could be ranked for degree of introversion for each of the three judges. These rankings, by three different judges, show inter-correlations of the values of 0.74, 0.58 and 0.23. The *Ss* were also ranked by giving each the combined net score assigned him by all of the judges. This rank, except for the last two cases on the list, was precisely the same as that secured by averaging the ranks given by the different judges.

For 10 of the *Ss* measures of susceptibility to a three-grain dose of caffeine alkaloid were computed, these being the net effects, over and above that of the control dose, in the 'intensive' part of the original experiment. Three tests were considered, these being Tapping, Mental Addition, and Color Naming.

In Table II are given the ranks for degree of introversion and the percentile effects of the caffeine dose, for every *S* and for each test. In all these tests the general effect of caffeine was found to be stimulation. A plus score for susceptibility means an effect in this direction. A minus means an opposite effect. Hence the greatest negative scores presumably indicate the least susceptibility.

The most susceptible subjects are seen to be Nos. 15, 12 and 11, who stand precisely in the middle of the list for introversion-extraversion. The most

Introversion-Extraversion Questionary

(Based on Jung, McDougall, Nicoll, Kempf, Allport, Wells and Freyd)

Instructions: Make an X of the line under the appropriate degree of possession; then record the totals for every column and graph as a surface of frequency showing net introversion and extraversion tendency.

	Characteristics	Degree of Possession				
		++	+	?	−	−−
1.	Is slow and reserved in the expression of emotions	/	/	/	/	/
2.	Finds difficulties in adequate self-expression	/	/	/	/	/
3.	Inclined to brooding and reflection before action	/	/	/	/	/
4.	Absorbed in internal conflicts, when under strain	/	/	/	/	/
5.	Blushes easily, easily rattled, very self-conscious	/	/	/	/	/
6.	Turns persistently away from contact with the world	/	/	/	/	/
7.	Is reserved, guarded, watchful, hard to understand	/	/	/	/	/
8.	Hides everything personal, dreading emotional exposure	/	/	/	/	/
9.	Reveals self only to intimate friends, and but partly	/	/	/	/	/
10.	Is self-depreciatory, keen and serious critic of self	/	/	/	/	/
11.	Approaches everything critically and suspiciously	/	/	/	/	/
12.	Full of anxiety and fear, about the present and future	/	/	/	/	/
13.	Prefers intellectual amusements to athletic games	/	/	/	/	/
14.	Dwells largely in more ideal realm of imagination	/	/	/	/	/
15.	Takes everything too personally, with self-reference	/	/	/	/	/
16.	Avoids friendly contacts, and has few intimate friends	/	/	/	/	/
17.	Avoids all occasions requiring public speaking	/	/	/	/	/
18.	Dislikes any act of personal persuasion or selling	/	/	/	/	/
19.	Prefers to work alone, rather than with other people	/	/	/	/	/
20.	While depreciating self, assumes outward air of conceit	/	/	/	/	/
21.	Prefers to keep in background on social occasions	/	/	/	/	/
22.	Reticent and retiring, shrinks when facing a crisis	/	/	/	/	/
23.	Conservative and painstaking with dress and property	/	/	/	/	/
24.	Very sensitive, readily hurt, inclined to self-pity	/	/	/	/	/
25.	Keeps personal diary, fond of writing about self	/	/	/	/	/

TABLE II

INTROVERSION RANK AND CAFFEINE SUSCEPTIBILITY

Subject	Rank Introversion	Percent Effect of Caffeine on			
		Tapping	Adding	Color-Naming	Total
1	1	0.6	0.6	9.3	10.5
4	2	7.1	− 0.8	8.4	14.7
8	3	− 3.9	0.3	1.1	− 2.5
9	4	− 0.1	2.7	− 2.9	− 0.3
15	5	4.4	13.3	33.6	51.3
12	6	12.4	1.3	13.6	27.3
11	7	4.2	8.6	3.7	16.5
6	8	− 4.1	2.7	− 1.4	− 2.8
7	9	− 2.8	− 1.3	7.3	3.2
16	10	− 4.0	5.1	2.4	3.5

resistant subjects, Nos. 8 and 6, are the one near the beginning, the other near the end of that list. There is clearly no definite relationship between the two characteristics shown by these results.

As a matter of fact only the first of these 10 *Ss* was assigned a positive or absolute net score on the questionary. All the rest had negative scores, thus being rated as more or less absolutely extravert. Twelve of the 15 showed this character. It is very likely true that, if there are introverts or extraverts, one tends to secure the latter rather than the former as coöperating subjects in an experiment of this sort.

In the light of the absolute net scores, then, we should perhaps speak of degrees of extraversion rather than of the opposite. However, until some one has fixed an absolute zero for such traits, it is probable that all we have in any case is a rank order for a certain openness or shutness of attitude. The general fact thus remains that the most susceptible subjects were neither markedly extravert nor introvert, but approach closely to the balanced or equivocal status in this regard.

McDougall has suggested that there are two groups of common drugs, the one illustrated by alcohol, to which the extravert is susceptible, the introvert being more resistant. The other group, with opposite psychological effects, is typified by the alkaloids, of which caffeine is an example. Presumably the introvert should be found most susceptible to drugs in this second group, and the extravert resistant.

We have secured only one absolute introvert (as rated). But he is less susceptible than half of those rated more extravert than he; and among those given absolute extravert ratings, there is no relation between degree of this trait and susceptibility to caffeine.

We have not been able therefore to confirm this aspect of the ingenious theory of McDougall concerning the relationship of temperament to drug resistance. Nor have we disproved it. Our questionary may have been inadequate; the judges estimates may have been wrong; the dosage may have been too light to reveal characteristic differences in susceptibility; the tests themselves may have been imperfect; our measures of susceptibility may be questioned—based as they were on the percentage change in each subjects performance, these performances being themselves on different levels of excellence.

We have, however, put the theory to an experimental test. The possibility of a connection between temperament and various forms of susceptibility and resistance is of such great practical and theoretical importance that the topic merits further exploration.

Columbia University By H. L. HOLLINGWORTH

CONCERNING THE MEANING OF THE TERMS *DIOTIC* AND *DICHOTIC*

The use of the terms diotic and dichotic in the literature of auditory localization has been somewhat confusing. The usage at first was with reference to frequency in binaural stimulation; later usage extended the reference to include also other physical aspects; and latterly these terms have been applied with even more variation in the description of conscious effects in binaural hearing. Nevertheless it seems that these words retain univocal meanings.

When these two terms are employed with reference to conditions of binaural stimulation, *diotic* refers to conditions in which the stimuli at the two ears are the same, and *dichotic* to conditions in which the stimuli are different, in regard to the particular physical factor or factors under consideration. When the reference is to conscious effects in binaural hearing, *diotic* refers to effects in which the sensory contents corresponding to the two ears are the same, and *dichotic* to effects in which the sensory contents are different, as regards the particular conscious aspect or aspects under consideration.

Thus, whether the reference be to conditions of binaural stimulation (*i.e.* frequency, energy, temporal disjunction, or spatial characteristics, considered separately, or in combinations, or collectively) or to conscious effects in binaural hearing (*i.e.* pitch, tonality, loudness, extensity, or successivity, considered separately, or in combinations, or collectively) *diotic* indicates a condition of sameness or identity, and *dichotic* a condition of difference or non-identity, as regards the particular aspect under consideration.

It may be that the confusion has derived not so much from the variable use of these terms as from the fact that the intended reference has not in every case been clearly indicated. Take the case, for instance, in which the binaural stimuli are the same as to the aspect of intensity and different in regard to the other physical aspects. In this case the stimuli are both diotic and dichotic at the same time, and if the aspect under consideration is not clearly indicated, *diotic* might be taken in the sense of *dichotic*, or *dichotic* might be taken in the sense of *diotic*. This complication would obtain in any case—whether in the description of conditions of stimulation or of conscious effects—in which the stimuli or sensory contents are both diotic and dichotic at the same time. Or the reference may be complicated by the use of terms, such as sound, tone, and intensity, that have been employed with double connotations in the description of conditions of binaural hearing.

Thus *diotic* refers to conditions of sameness or identity, and *dichotic* to conditions of difference or non-identity, in regard to the aspect under consideration, whether it be a physical or a conscious aspect in the description of conditions in binaural hearing. If the aspect under consideration is clearly indicated, there would seem to be little chance for equivocality.

Harvard University OTIS C. TRIMBLE

Wilhelm Betz

The death of Wilhelm Betz, on August 19, 1930, has occurred more or less unnoticed by the mass of psychologists, particularly those outside of Germany. In this his death has not been very different from his life; therefore it seems fair to devote a few lines to the memory of this man who might have become as well known as many another psychologist, had he been so inclined. Betz was a professional psychologist without making Psychology his profession: he lived as a private gentleman outside of academic circles, and without personal ambition. While this attitude gave him a certain freedom from constraints, intellectual and otherwise, and thereby fostered the originality of his work, it kept him outside the recognized circles both of academic institutions and of doctrinal schools.

He was born in Mayence in 1882, studied physics and took his Ph.D. in that science (1905), then went to London to study statistical methods under Karl Pearson. On his return to Germany he vacillated between a resumption of his work in physics and an occupation with the problems of science in general. This led him into the field of the psychology of thought, and in this he has made his most original contributions, which were finally brought together in his book on the *Psychologie des Denkens* (1918). His theory is an attitudinal, dynamic theory of thought. With great gusto he investigated the functional meaning of a number of conjunctions like 'but,' 'yet,' and coined the terms 'butness' (*Abrigkeit*), 'yetness' (*Dochigkeit*), etc. The book was a real contribution to this most difficult part of our science. It was the result of long labor; two papers published in 1910 and 1911 bear witness to this. At this time he also published a monograph on the method of the correlation calculus, which at that time was not very familiar to German psychologists.

The war and subsequent inflation completely destroyed his financial position. For some time he was supported by the *Notgemeinschaft der deutschen Wissenschaft;* for a time he also undertook some lexicographical work to provide the necessary means of subsistence. At the end of his life he was looking forward to support by an American endowment. During all these hard years he worked with great energy and zest at a book on the psychology of moral opinions and social institutions, which was to bear the title: *Love, Dignity, and Morals.* On the very day of his death he finished a chapter of this book.

Psychology mourns the loss of an original thinker who devoted his energy to psychology for no other reason than that of gaining new knowledge.

An extract of the bibliography of his writings follows:

(1) Dissertation: *Methode zur Bestimmung der Dicke und optischen Konstanten durchsichtiger Metallschichten*, Leipzig, 1905, 30 pp.

(2) Vorstellung und Einstellung, *Arch. f. d. ges. Psychol.* 17, 1910, 266-296.

(3) Vorstellung und Einstellung II; Über Begriffe, *Arch. f. d. ges. Psychol.*, 20, 1911, 186-225.

(4) Über Korrelation, Beiheft 3 zur *Zsch. f. ang. Psychol.*, 1911, 88 pp.

(5) *Psychologie des Denkens*, Leipzig, 1918, 351 pp.

(6) *Zur Psychologie der Tiere und Menschen*, Leipzig, 1927, 206 pp.

Smith College K. Koffka

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Science of Psychology. By RAYMOND HOLDER WHEELER. New York, Thomas Y. Crowell Co., 1929. Pp. xvii, 556.

This is the first introductory textbook in general psychology written from the viewpoint of configurationism. It is perhaps the most comprehensive and the most difficult of recent books of this sort. The latter characterization is merited because it revolves about a somewhat abstract theory of behavior, to which experimental problems and facts are made subservient. Configurationism has been a magic word in the realm of psychological theory during the past decade, tending to displace behaviorism which was the fashion in the preceding decade. As is the case with all movements, the *gestalt* variety will no doubt eventually become, as someone as well said, just psychology. At present it still is very much a movement, and in many respects a very salutary one. On the theoretical side it has been the source of much inspirational reorganization and reinterpretation of psychological data, as Wheeler's book amply illustrates.

In the first place, we note that "instead of beginning with sensations and reflexes and ascribing to them the conventional systematic meanings, the book commences with the whole of which the human being is psychologically a part, namely the social group. . . . The later chapters are devoted to sensation and the nervous system. The text reverses the customary order of subject-matter because it adheres throughout to principles in many respects the opposite of those around which previous systems have been written." In conformity with this program, we find the following order of topics in the text: social behavior, 2 chs.; intelligent behavior, 2 chs.; emotive behavior, 3 chs.; learning behavior, 3 chs.; simple reaction and observational behavior, 2 chs.; observational behavior, finer aspects (sensory reactions) 2 chs.; the nervous system in its relation to behavior, 2 chs.

Psychology is defined in the introductory chapter as a "study of conscious behavior and of the conditions under which that behavior takes place. . . . human psychology as the study, first, of social behavior with its various limited and specialized activities, and second, of those forms of behavior which can be abstracted from it." There are a few well-chosen remarks on the general conditions of behavior, the sources of psychological data, the nature of scientific method, with applications to psychology, and a sketch of the history of psychology, necessarily fragmentary. The interesting thing, to the reviewer at any rate, about the discussion of psychological methods is the ingenuous manner in which the author both discards and preserves the method of "structural analysis." He tells us that this procedure cannot give us "prediction and control, but these results none the less are useful in identifying various kinds of abstracted processes like choosing, comparing, recognizing and reasoning." This is a pretty weak justification of the attempt to harmonize (by implication at least) two incompatible viewpoints.

The author's strategy in beginning with social behavior, because the individual's behavior under social conditions is unrestricted, seems illusory. He is forced by the paucity of experimental methods and facts to resort to rather superficial descriptions of social phenomena and to vague generalizations whose bases are often simply opinions of the author or of sociologists. Wheeler has perhaps done as well as anyone could do, at present, with the topic, but the student would profit more from his discussion after knowledge of his theory of behavior derived from less complex and more easily verifiable examples. It is not essential to an introductory textbook that a theory of behavior be translated bodily into a theory of instruction.

The author rejects four historical explanations of social behavior (suggestion, imitation, instinct and sympathy) and substitutes for these (1) the law of of configuration, and (2) the law of least action. The first of these new principles states that "any reaction of the human organism-as-a-whole is a unified response made to a total situation of some kind, and if to a specific detail, always to that detail in relation to other details." The law of least action implies that all responses are initiated by tensions or strains of some sort and that the reactions constitute activity directed in a line of least action. "In terms of the law there is no ineffective or waste motion, no trial-and-error. It is a question of degree of configurational organization. Poor and good performances, alike, follow the law under different sets of circumstances." I suppose this means that the reaction which an individual makes to a situation is the only reaction possible under the circumstances. If this is the correct interpretation there is nothing original about the 'law.' It really is a fundamental postulate underlying all scientific psychology. The distinction between wasted motion and a 'poor' response is a needless bit of sophistry; the term 'trial-and-error' does not mean an arbitrary, lawless sort of reaction and it is perhaps as good as any other to designate the phenomena in question.

Intelligent behavior is treated first from the standpoint of measurement of performances and the interpretation of such measurement (Indirect Methods). With respect to the nature-nurture question, the author is inclined to attribute rather more importance to environmental than to hereditary influences. In fact, from his viewpoint the distinction between heredity and environment is artificial; we have controlled conditions (environment) and uncontrolled conditions (heredity). There is great emphasis upon results which seem to support his view that performance or intelligent behavior is largely a product of controlled environmental conditions. The author attempts to explain away results upon the constancy of the I.Q., as well as statistics upon degenerate families, which support the view that innate factors are very important, if not the most important, conditioners of level of intelligent behavior. He quite correctly makes the point that the two sets of conditions cannot in any real sense be dissociated, but he proceeds to do violence to his own thesis by attempting to establish in several instances the generalization that "improvement in environment conditioned a definite improvement in intelligence."

The second chapter on intelligent behavior (Direct Methods) includes (1) a descriptive review of various studies and theories of adaptive behavior in lower animals, (2) an hypothetical account of the phylogenetic development of in-

telligent behavior in man, (3) a survey of the ontogenetic phases of mental growth, and (4) an analysis of intelligent behavior from the introspective viewpoint. This wide range of phenomena is interpreted by the author in terms of the following 'laws': (1) a state of tension which results in (2) an act of perceiving a goal in relation to the whole stimulus situation (in lower animals the goal may not be realized but it nevertheless exists), and (3) activity in the line of least action, the end of which is the resolution of the tension. There is no such thing as mechanically tropistic or instinctive behavior. When an organism behaves "unintelligently" this simply means that we are judging it with respect to one criterion of success (goal), whereas the animal is reacting to another goal. All behavior, according to this view, is directional, configurational, adaptive. The difficulty with such an all-inclusive formulation is that it becomes, by the very fact of its generality, relatively devoid of meaning. Neither the 'mechanical' not the 'purposive' descriptions change any of the facts of behavior.

Emotive behavior is classified into four groups: (1) "being pleased or displeased," (2) a more complex group, "fear, anger and love," (3) still more complex, hatred, jealousy, grief, awe, pity and numerous others, and (4) "religious, moral and aesthetic sentiments." The first chapter deals with the history of the problem of emotion and of the relation of emotional behavior to feeling and instinct. Instinct and inherited tendencies are alike discarded because, for one thing, they assume the thing they are postulated to explain. "The problem of behavior involves the problem of inheritance only to the extent in which behavior is conditioned by organic structure." But even organic structure may not be due to inheritance. Although Wheeler admits that the constitution of the germ cell determines "to some extent" the growth of various structures within the organism, he thinks, nevertheless, that "it is possible that the environment both of the ovum and of the embryo, being very much alike in members of the same species, determines many of the traits which have been attributed to heredity." This is a pretty weak argument against heredity, for it naturally raises the question as to why the 'environment' of the ovum and of the embryo is very much alike in members of the same species. The general nature of the organic environment to which, presumably, Wheeler refers is fundamentally conditioned by the physico-chemical organization of the fertilized germ cell. Hence Wheeler's argument really favors the view which he wished to discredit. In general, the data presented in the book suggest many possible explanations which are alternative to those ordinarily accepted, the author's phobia against heredity being a prominent example.

A survey of experimental methods of studying emotive behavior, and the application of Wheeler's two trustworthy laws to the explanation of the phenomena constitute the subject-matter of the other two chapters on this subject. What most psychologists refer to as the problem of motivation is handled rather incidentally by Wheeler. The thwarting of any goal-activity (but are not all activities goal-activities?) produces tension, and tension is resolved by activity which occurs in the line of least action. He doesn't like the terms 'urge' or 'drive,' because they are substitutes for instincts. "These so-called drives are goal-activities, and these goal-activities are tensions demanding

resolution, not because of their heritable character or innateness, but in accordance with a fundamental physical law and its conditions, the law of least action." This seems to confuse the tension (or drive, to use a synonym) with the activity by which the tension will be relieved. The confusion of these two concepts is undoubtedly responsible for much of the fruitless controversy over 'instincts,' which has not yet disappeared from psychological literature.

The section on learning revolves, as do all others in the book, about the configurational hypothesis. Learning is defined as "that behavior in terms of which the individual extends his insight into a given situation, and increases the complexity of his reactions with respect to a certain goal." The goal, as an anticipated object, plays an important part in Wheeler's discussion. Poor learning means that the goal is imposed upon the animal and that he fails to react to it. He has goals of his own which condition his performance. (On a preceding page this statement is contradicted, for he tells us that in a new situation "the human learner grapples *blindly* with his problem.") Experiments upon the effects of attitude and of knowledge of results upon learning efficiency are cited as evidence of the existence and efficacy of the goal in such situations. Support for the law of configuration is found by the author in experimental results which show the superiority of logical as compared with rote memory, and in results which favor the whole as against the part method of memorizing. The configuration hypothesis is contrasted with several association principles as an explanation of learning. The association theory is criticized upon logical grounds for assuming that learning begins with unorganized experiences and movements, whereas in reality "any event of behavior, no matter how complex, is already an integrated performance the first time it takes place, whether it be considered as a mental process or as an overt act." Association principles seem to imply that the individual is a passive victim of mechanical agencies, whereas in reality association comes with learning, not before it; with experience, not prior to it. Wheeler fails to note that the rôle which he assigns to the stimulus-pattern makes the individual almost as passive as any association theory. The two main conditions of learning are stimulus-pattern and maturation. But maturation is really tautological, a question-begging epithet, as the author himself practically admits. Maturation involves, we are told, a differentiation of energy patterns or system of stresses within the nervous system, which are organized as they differentiate. Or it may be something analogous to the formation of physiological gradients. It really is a synonym for learning.

The chapters on observational behavior, including simple reaction, attention phenomena, perception and the sensory processes, are uniformly good. The author has included a wealth of experimental material and has presented it in an exceptionally clear manner. The concept of attention is criticized as failing to account for the diverse phenomena usually included in this category. In this connection the author denies that 'clearness' is an attribute of 'consciousness.' The *gestalt* doctrine of perception is illustrated by numerous experiments. The familiar criticisms and experimental refutations of the 'constancy-hypothesis' are effectively presented. Investigations of apparent movement—important in the history of configurationism—are discussed rather fully. Sensory re-

actions are accorded more space than they have received in most of the recent textbooks. In general, the facts are well-organized and up-to-date. One fails to find, however, any mention of the work of Wilkinson and Gray in connection with the author's criticism of the resonance theory of hearing. The statement that the fibers of the basilar membrane are not stretched and hence cannot act as resonators would seem to be ill-founded if the spiral ligament exerts tension on these fibers in the differential manner suggested by these two writers.

The two chapters on the nervous system in its relation to behavior are the best the reviewer has seen in an introductory textbook. Wheeler has done a valuable service in collecting together the multitude of facts which have resulted from many types of work on nervous functions. On the theoretical side his rather literal analogy of nervous action to 'stresses' and 'tensions' in the realm of mechanics is almost pure speculation, the ultimate value of which cannot be determined at present.

A final evaluation of the speculative portions of the book must wait results of experimental work in many fields. Wheeler definitely set out to write a system of psychology, realizing that any system must of necessity be radically modified as factual material accumulates. Considered from the viewpoint of how well the configurational theory of behavior is applied by the author to the diverse phenomena in the field of psychology, the book is a carefully executed, finished work.

Vanderbilt University LYLE H. LANIER

Contributi del Laboratorio di Psicologia e Biologia della Universita Cattolica del Sacro Cuore. Serie Terza. Milano, Societa Editrice "Vita e Pensiero," 1928. Pp. viii, 436.

The present volume entitled Contributions of the Laboratory of Psychology and Biology, third series, constitutes Volume IV of Series 6, Biological Sciences, of the publications of the Catholic University of the Sacred Heart situated in the city of Milan. It contains the studies completed in the laboratory during the academic year of 1926-27, under the direction of Professor Agostino Gemelli, We indicate below the titles of the ten studies comprising the volume and a brief suggestion of their contents.

(1) Lodovico Necchi, Richerche medico-statistiche sui fanciulli anormali.

An elaborate statistical organization (71 tables, 17 graphs) of data on abnormal children based on biological variations.

(2) Guiseppina Pastori, Pineali accessorie e relazioni tra gli organi pineale e subcommensurale.

Histological study of the epiphysis cerebri.

(3) Guiseppina Pastori, Qualche osservazione sulle patogenesi delle calcificazioni pineali.

Discussion of the problem of the calcification of the epiphysis cerebri.

(4) Giulio Castiglioni, Recerche ed osservazioni sull 'idea di Dio' nel fanciullo.

A study of what might be called the philosophical attitudes of children in the manner made familiar to American students by the work of Piaget. The author gathered replies from Catholic children in the primary school grades to

questions designed to elicit their conceptions concerning the relation of God to the world in general and to man. The results naturally are faithful indications of the family backgrounds of the children.

(5) G. M. Vacino, Contributo alla studio delle immagini eidetiche.

In this study the author uses various forms of materials in order to determine both the existence of eidetic types and the kind of material upon which subjects can be classified as eidetic. He concludes that eidetic imagery is unstable and constantly undergoes transformations.

(6) Agostino Gemelli, Introdizione alla studio della percezione.

This article is a criticism of the *Gestalt* theory of perception. Gemelli asserts that both upon the basis of general psychological considerations and various experimental results perception must be regarded not as an intuitive awareness of a totality but as a definite organization of discrete elements. The author proposes a series of laws according to which this synthesis is made. These laws are as follows. (1) The law of minimal means. Materials of sensory origin are organized according to the greatest economy. (2) The law of the relative functional autonomy of the single element of perception. Mental complexes are made up of more or less stable independent elements or aspects. (3) The law of functional unification of all the elements of perception. The sensorial elements are made into a single complex or object. (4) The law of the definite character of perception. The indefinite single sensory data assume a determined and precise form. (5) The law of the rectification of sensory data and of the relative constancy of intuitive organization. Sensory data are centrally transformed. (6) The law of the finality of intuitive organization and of the significance of the objects of perception. We do not perceive only intuitive organizations but objects which have significance. The intuitive organizations are intimately linked with meaning or significance. An abstract of a French translation of this article is given in Psychological Abstracts, 1929, no. 59.

(7) Alessandro Gatti, Di una illusione nel campo delle sensazioni cinestesico-tattili.

A preliminary study of an illusion in the kinaesthetic-tactile perceptual field. The author by means of cardboard rectangles and triangles produced an illusion situation similar to the Zöllner optical illusion; parallel lines perpendicular to the base within a triangle converge toward the base. The result is explained as based upon habitual convex movement in tracing outside rectilinear lines.

(8) Alessandro Gatti, I complessi visivi per presentazione successiva delle singoli parti.

In the present article Gatti reports the results of a study consisting of two parts. In the first, he presented to four subjects figures consisting of series of inscribed triangles and polygons, and then imposed upon them either a circle or a square with the resulting illusion of deformation (Zöllner). He introduced various temporal relations in the presentation of the parts, as well as other changes during the course of the experiment. In the second, he obtained primarily from one subject the report that when the circle or square was perceptually presented and the other stimulated imaginally by a substitute nonsense name, the illusion was experienced with most combinations. Gatti

concludes that the deformation obtained does not depend upon physiological conditions but exclusively upon purely psychological conditions linked with the act of perceiving.

(9) Alberta Zama, La percezione tattile della forma.

The author undertakes to study the tactile perception of forms by means of geometric patterns and shapes of objects cut out of cardboard. He raises the question not only whether such objects could arouse ideas that are exclusively kinaesthetic-tactual but also how such mental complexes are formed. At least in one subject, he obtained definite results, and that in a person who is predominantly visual. As to the formation of such ideas Zama concludes that they arise from situations that are primarily visual.

(10) Agostino Gemelli, Contributi allo studio della percezione: IV. Il comparire e lo scomparire della forma.

In this article Gemelli brings forth experimental results which he regards as demonstrating the sixth of his laws of perception which are referred to in (6). As over against the contention of the *Gestaltists*, who would stress the stimulus or contact side of perception, the author argues for the importance of the meaning imposed by the mind upon the sensory elements, furthering their organization into perceptual objects.

Indiana University J. R. KANTOR

Viljandet i dess förhållande till Jaget och Aktiviteten. By EINER TEGEN. Upsala, Almquist & Wiksell, 1928. Pp. 568.

This book is a companion volume of the author's *Moderne Willenstheorien, Eine Darstellung und Kritik*, the first volume of which appeared in 1924 and the second in 1927. But whereas the *Moderne Willenstheorien* comprised a review of important modern theories, this volume is devoted to a systematic presentation of problems inherent in those various theories, an attempt at a synthesis of their various points of view, and an effort "to find a way out of the labyrinth and arrive at a sustainable conception of the will." That the author is not so presumptuous as to assume his conclusions will constitute a final or closed system is indicated by his recognition that the subject under consideration is ambiguous and difficult of apprehension, that its meanings are changing, and that the very subject itself raises questions involving the most varied philosophical and psychological problems.

The concept of volition as action or activity is treated as one of quite general acceptance. Of those whose theories favor that point of view, or approximate it, are mentioned Spencer, James, Münsterberg, Wundt, Bain, Fouillée, Jodl, Höffding, and Lipps. Then follows an analysis of the psychic condition we call will, five components of which are considered in detail. There are, (1) the time element of the psychic condition, (2) the time element of activity, (3) apprehension, the feeling of effort, (4) the unity of the psychic condition, and (5) the importance of consciousness (consciousness of). Consciousness is viewed as a universal attribute of the psychic state, and implies an Ego, and self-consciousness. Various theories based on feeling, desire, choice, decision are reviewed. The author's own theory of volition is developed in particular detail. More than two hundred pages are devoted to this, and out of this mass

of discussion the following salient features emerge. Volition is a psychic condition, a state of attention, which implies that along with an attentive "consciousness of" having a distinct character there also enters a "moment of feeling." This "consciousness of" is an apprehension of action in which the active Ego receives "the life and color of feeling." "One can therefore say that in volition a 'consciousness of' enters, in which feeling (ego) is placed in relation to a distinct happening." Volition is not itself the "consciousness of," but rather in volition we have an attentive "consciousness of." The complexity of feeling is recognized, and is developed as a co-existence of the quality of feeling and the apprehension of a distinct happening, involving the element of time. Volition itself is an indissoluble quality, "a distinct element of the soul."

The book contains a carefully organized and detailed table of contents, but there is no index of subjects. No bibliography is provided, but numerous and specific references appear in the foot notes. As a theoretical treatise this work is preëminent. Even the findings of such experimental psychologists as N. Ach, A. Michotte, and E. Prüm are subjected to theoretical scrutiny. It provides an important contribution to psychological literature in a field in which American psychologists have not distinguished themselves. That this contribution may be more pronounced in making available a vast amount of material than in any finality of conclusion in no way detracts from its value. It may, however, be rather disconcerting for some of us to find that reference to the soul has not altogether disappeared from psychological literature.

Syracuse University ERNST THELIN

Les dettes de guerre et la constitution d'un fouds des professionnels intellectuels et de la paix. By S. SCHIFFER. Introduction by Justin Godart. Paris, Jouve & Co., 1929. Pp. v, 37.

This is a pamphlet circulated in the interest of the establishment of a fund for international coöperation by leaders in science, literature, and art. The pamphlet sets out in an appealing manner the distress that the war has wrought among intellectuals, especially in Europe, and traces briefly the historical beginnings toward the formation of an International Institute for Intellectual Coöperation. It points out the dangers to society which are resulting from the economic handicaps imposed by the Great War upon intellectual leaders, who, though considered by the populace to be well able to take care of themselves, are really so much occupied normally, and rightfully, with their several researches (of great value to humanity in general) as to neglect making adequate provision for their own economic needs. The inevitable consequence is that the advancement of knowledge suffers. In recent years science has demonstrated the possibilities of universality and impartiality in its contributions to humanity, and its right to have an important rôle in the management of human affairs.

It is proposed to raise a very considerable fund toward the maintenance of an international institute for intellectuals and for peace. Such an institute, it is pointed out, must have a permanent home with adequate equipment and an enormous library so that it can be a sort of super-intellectual center of the world, affording grants for research, opportunity for travel and study and for

contacts and coöperation among intellectual workers of all nations, and intensive, impartial study of world problems. It suggests that a part of the money collected on war debts due the United States—about which there are, as is well known, differences of opinion both as to obligations and possibilities of payment—might well be set aside as a basis for this fund. It is also indicated that an institute of this sort working for world peace and comity would greatly reduce the expenses of different nations which are now competing with one another toward keeping up great navies and other preparations for war and that it would thereby really not be an additional expense but would work toward reducing huge expenses otherwise inescapable. Several different sources from which bequests might be obtained are indicated. It is made clear in this connection that the fund should be large enough to accomplish efficiently the work of the institute a considerable part of which is to stimulate and encourage intellectual pursuits, recreations, friendly competitions, and other similar activities over the entire world, so that the benefits of intellectual pursuits could be widely distributed toward solid, peaceful progress.

This project has been discussed in various journals and seems to merit the coöperation and the encouragement of psychologists, especially since psychology is just in an early stage of making important contributions to problems of health, industry, education, art, the reduction of crime, etc. It is important that the project be considered thoroughly from all angles and talked about everywhere so that its possibilities as contrasted with the wastes of periodic wars and preparations for war (or defense) may stand out.

J. P.

The Child's Conception of the World. By JEAN PIAGET. With eleven collaborators. Translated by Joan and Andrew Tomlinson. New York, Harcourt, Brace & Co., 1929. Pp. ix, 397.

This is the third of the author's four studies in child logic to appear in the International Library in translation. Appointed director of the *Institut J. J. Rousseau* in 1921, Piaget has issued in rapid succession, in collaboration with fellow-workers, *Le langage et la pensée chez l'enfant* (1923), *Le jugement et le raisonment chez l'enfant* (1924), *La réprésentation du monde chez l'enfant* (1926), and *La causalité physique chez l'enfant* (1927); the fourth and last reviewed in a recent number of this journal, but as yet inaccessible in translation.

The series as a whole represents a systematic attack upon the problems of child thought, along a wide front. A notable contribution to the psychology of the years 4-12, it contains matter which no psychologist or sociologist can afford to overlook. The present volume limits itself to an examination of the content and texture (rather than the logical mechanism) of experience, interest centering upon the degree of objectivity achieved by the external order at different levels of development. By a series of well-directed but informal queries, constituting in fact a new experimental method, childish notions as to the nature and origin of the sun, moon, and stars, clouds, lakes and mountains, night, dreams, names, etc., are skilfully exposed, with a minimum of tampering and damage to their original structure. The unique and *sui generis* character of the child's apprehension (foreshadowed in the work of Stern and Binet, and

in the egocentric traits of childish logic disclosed in earlier studies by the author) is clearly patent in the protocols, many of which are quoted in detail, in all their native quaintness and incoherence of phrasing. Starting with the naïve realism or *adualism* of Mach and Baldwin—hall-marked by its confusion of subject and object—childish experience is shown to pass through various stages of animistic and artificialist interpretation, as forecast in the rough by James and Sully. Only gradually, with the socializing of thought, does there emerge the adult notion of an impersonal, mechanically determined order; subject to frequent lapses to purposive or finalistic levels.

In the general profile of childhood thus projected there is little that is wholly novel; little, in fact, that has not been common currency of instruction for a quarter-century. Piaget's distinctive merit lies in corroborating on an extensive scale the brilliant guesses of earlier observers—Sully, James, Hall, Binet—by a methodology of his own. Because of the natural solipsism of the child and lack of incentive to communication, the 'free observation' of the earlier studies is judged inadequate to the work at hand. It is therefore abandoned (except as the starting-point of inquiry) in favor of a type of informal but directed questioning, an adapted 'clinical examination' method. Rules and safeguards are painstakingly developed, and the advantages of the new procedure over the more limited and falsifying routine of tests and questionnaires convincingly presented.

Five types of response are carefully distinguished: the answer at random, romancing, and suggested, liberated and spontaneous conviction. Uniformity of response on certain age-levels, independent of environment, and systematization, or stability as gauged by later approach from a different angle, are employed as criteria of reliability. Though the data utilized are drawn from a larger number of children than in some of the earlier studies, the treatment of results is qualitative and interpretative rather than statistical, as is fitting in a pioneer investigation. The author's insight and originality are displayed especially in those sections in which he assimilates his findings to those of workers in related fields; massing his materials so as to reveal the natural stages of progression.

The discussion of data is organized under three heads—Realism, Animism and Artificialism. Sections which will perhaps best repay cursory reading are Ch. II, on Nominal Realism—a caption borrowed from Rougier, to signify the confusion of the name with the essence of the object; Ch. III, on Dreams, revealing a childish belief in the purposiveness of the dream; Ch. IV, The Origin of the Idea of Participation, marshalling instances of child magic which display analogies with the primitive attitude posited by Lévy Bruhl; Chs. IX and X on Metereology and the Origin of Trees, etc., in which resemblances to the primitive physics of the Pre-Socratics is traced; and Ch. XI on Artificialism.

Though bent on rendering an unbiased account of the child mind, in terms strictly of itself, Piaget is not indifferent to parallels in related disciplines, enriching his discussion with analogies derived from psychiatry, semantics, anthropology, and in turn providing suggestive matter for workers in these fields. Holding that both Ribot and Freud offer too artificial and mechanical an account of animism (the endowing of external objects with activity and will),

he himself provides a simpler solution. The child's confusion of movement with purposiveness is not a case of systematized 'projection' or intrusion of mind into matter, but an instance primarily of *indissociation*. The notion of the universe as a network of intentions, a social order amenable to human influences, is a direct derivative of the primitive continuum of infantile experience, which is at once pre-psychic and pre-physical. There is therefore no need to invoke in explanation the complicated mechanisms of transfer and displacement, or of compensation and defence, characteristic of the compulsion neurotic.

At certain turns of the argument, incredulity is likely to assail the reader. Doubt as to the spontaneous displacement of animism by artificialsm, in the absence of specific religious or deistic tuition, is, however, speedily allayed by Piaget's ingenious marshalling of evidence, from his own records and those of Sully, James, Freud, Rasmussen, Cramaussel. Artificialism, or the reference of all natural objects to a human or anthropomorphic agency, originates, it appears, in curiosity concerning birth, and is fed by the child's observation both of himself as a natural cause, and of his dependency upon his parents. In its final phase, it approximates the notion of Nature as the supreme artificer singled out by Brunschvicq, in his recent study, as the salient feature of Aristotle's physics. Instead, therefore, of religious teaching influencing his thinking, the child is found to assimilate it to his autonomous thought, distorting and altering it. Genuine religious emotion, Piaget believes, is possible only by transfer and sublimation, as the child's spontaneous deification of his parents declines.

In respect to certain other matters, scepticism is less easily dispelled. In spite of Piaget's scientific caution and acumen, data pass muster as evidences of persisting animism which seem to the reviewer mere artifacts of the method of reiterated quizzing. Furthermore, the author appears now and again to read into his records more significance than his own earlier findings with respect to the pre-casual use of *why* and *because* through the sixth year, and the casual linkage of statements through juxtaposition rather than logic (*syncretism* and *transduction*) would seem to warrant. It is especially difficult, in the light of fairly fresh and definite memories of the sixth and seventh years, to view the childish statement that the sun and moon follow us as we move from place to place as more than a *façon de parler:* too slight in import, certainly, to bear the weight of the whole structure of childish animism and participation (pp. 214-215).

One wishes that Piaget had recorded more meticulously the social status, schooling, and contacts with cultured adults of the children quoted; since socialization is admittedly an important factor, under which sophistication and depersonalization of the world set in fairly early. While he reports results from a fairly wide geographical range, the application of his method to American children of varied social strata, age-levels and intelligence differences is eminently desirable as a check upon the universality of his conclusions. In corroboratory work, moreover, attention should be devoted to securing a greater uniformity of *Einstellung* than is apparent in accounts of the method as used at the *Maison des Petits*.

Cornell University ELSIE MURRAY

Modern Scientific Knowledge of Nature, Man, and Society. Edited by FREDERICK A. CLEVELAND, with 14 Collaborators. New York, The Ronald Press Co., 1929. Pp. viii, 592.

The purpose of this volume, we are told in a foreword, is to present in thirty-one short chapters a vision of the world in which we live as seen by men of science. Both the college freshman or sophomore and the reading public require orientation that will enable an "individual to see the world as it is and map out his mental course." This is attested by the fact that "millions of volumes" have been sold during the last ten years on general science, history, and philosophy. Some idea of the scope of this book will be gained from a mere statement of the main topics discussed in its four divisions. The first main division includes three introductory chapters dealing with the present-day need for perspective, pre-scientific cosmologies, and science, education and civilization. The second division, labeled inanimate nature, gives an account of the starry heavens above and the rocky earth beneath us, with a smattering of the newer physics and chemistry. Transition to animate nature in Part III is made by way of colloid phenomena since the life process is "a sequence of complicated colloid changes." Part IV deals with human nature and occupies the largest part of the work.

It will be of special interest to the psychologist to find out how his science is being ordered in the general scheme of things. He will be disappointed to find that from the chapters on psychology in this book one would never dream that psychology has been an experimental science since 1879 and that specific problems attacked in the laboratory by methods common to physics and chemistry form the backbone of his science. After perusing this book the non-psychological reader will be able to toy with such words as functionalist, structuralist, and behaviorist (but not, alas, configurationist!) even though he hasn't the faintest notion as to how the psychologist goes to work to solve his problems.

From the table of contents and the index of subjects one might be led to believe that this is a glorious work indeed. But let him look up a topic, or read through a chapter, and he will be sorely disappointed, for the book is so thin as to leave hardly any impression at all with the reader. We are told that we cannot all be physicists, psychologists, biologists, but we can learn to think as the men in these sciences do! What a hodge-podge and welter of knowledge that would entail. How naïve the belief on the part of the one who wrote this that all knowledge actually goes together in the present state of ignorance in which most of the sciences find themselves regarding fundamentals. He who would think as the physicist thinks would have to spend years in the physical laboratories—and likewise for psychological thinking. It is precisely this matter of thinking in certain terms that cannot be inculcated in a work of this kind.

The question which arises in the reviewer's mind as he contemplates this volume and hundreds designed for the same purpose is: Are they worth a student's time getting oriented as they will orient him? The answer of the scholar, one fears, will be that such attempts are hardly worth the paper they are printed on. Yet it seems as if something in the way of 'orientation' is necessary for the undergraduate and the public at large, even though there be no royal road to

knowledge. What is the fundamental difficulty with the hundreds of thousands of books flooding our markets and claiming to teach everything?

In this case the difficulty does not lie in the caliber of the contributors, some of whom, like Shapley, Giddings, and Newman, bear illustrious names in science. Nor can the fault be laid at the door of the editor who seems to have coördinated the works of the several authors in masterly fashion. The difficulty, it seems to the reviewer, lies in the magnitude of the task which is simply beyond the scope of one book, in the nature of the materials, which cannot be diluted beyond a certain point without losing its essence, and in the fact that a compendium of all the sciences cannot take the place of living contact with the data and methods of any one of them. Nor is the reviewer convinced that a smattering of all knowledge will give a fairer or truer picture of the world in which we live than a real knowledge of a single science gained by the sweat of one's brow. Do we need another Tennyson or Huxley to tell us that if we knew enough about a piece of chalk or the flower in a crannied wall, we should know the heart of nature? The last way leads us ever deeper into the world about us through a penetration which may be more restricted but ultimately, I believe, more illuminating.

Bryn Mawr College HARRY HELSON

Umriss einer analytischen Psychologie. Teil I: Grundlegung einer nicht-empirischen Psychologie. 2d ed. By WALTHER SCHMIED-KOWARZIK. Leipzig, Johann Ambrosius Barth, 1928. Pp. xii, 160.

What we commonly call psychology is, according to Schmied-Kowarzik, not one unitary science but comprises two sciences, empirical and analytical psychology, which are entirely different in logical form. The author calls 'analytical' all forms and systems of knowledge in which the goal of knowledge is the discovery of inner principles of relation. The judgments of empirical psychology (*Erfahrungsurteile*) are essentially assertorial, *i.e.*, they have a factual character; they refer to a present existence (*Dasein*) and their application is limited to individual items of definite number and time. Analytical judgments are, on the other hand, essentially apodictic, *i.e.*, they have the characteristic of *necessity;* they involve no present existence; and their application does not involve any existential number or time.

The general problem of Schmied-Kowarzik's analytical psychology is to reach apodictic certainty regarding mind. The particular partial problems are two; to discover the final, unanalyzable partial contents of consciousness and to bring them into systems; and to present schemata or models of complex experiences.

Knowledge in empirical psychology depends upon induction from observation of as many individuals as possible. In analytical psychology, however, one observation is enough. Schmied-Kowarzik believes, for example, that if he has *once* differentiated two items in his experience, the two are always and everywhere unlike and two. Observation of other individuals can give nothing to analysis which is not present potentially in one's own consciousness.

Since experiment is inductive and empirical, it has no place in analytical psychology. The characteristic method of analytical psychology is the con-

struction of models. Every experiment demands at least one repetition; every model holds once for all time. Among the experiences which the analytical psychologist seeks to present in terms of models are perception, empathy, apperception, the will, and affective ideas. The aim is not to show what *is* involved in any particular case, but what *must be* involved in all cases.

In this first volume, Schmied-Kowarzik presents only the fundamental principles of his non-empirical psychology. The working-out of the system is reserved for the second volume.

The book is, of course, representative of so-called 'philosophical psychology.' Yet it should not be forgotten that it is at the same time a representative work of a school which is exerting tremendous influence upon German psychology at large. Unfortunately the author's style is exhortatory rather than expository. The book provides, however, a comparatively simple introduction to this school of thought. The chapter dealing with the history of analytical psychology is perhaps the most useful; here, incidentally, is revealed Schmied-Kowarzik's own deep debt to Brentano, Meinong, and Husserl.

A criticism of the book by empirical standards is obviously out of the question.

Cornell University CHARLES WARREN FOX

Introduction to Abnormal Psychology. By V. E. FISHER. New York, The Macmillan Co., 1929. Pp. 512.

In this book the author attempts to present the point of view of the integration of mental experience as the basis for normal behavior, and he regards 'abnormal' as a matter of degree of general disintegration. This is, of course, no new theme as many psychologists and psychiatrists including Franz, Bridges, Conklin, and others have previously presented it.

The author has handicapped himself in explaining disintegration by incorporating into his text instinctive behavior, sub-consciousness, co-consciousness, introversion and extraversion. Although these terms are popular with many authors in the field of abnormal psychology they are not necessary for the thesis here in question. Since the usage and meaning of the terms have been discussed already in the psychological literature no further statement is necessary at this time.

The material is well organized and presented in such a way that it would be quite readily assimilated by the student. The only criticism that can be offered is that the student may arrive at the unfortunate conclusion that abnormal cases are easily classified and that the treatment and cure follow a 'simple formula' for each type.

The data used for illustrative purposes have been carefully chosen from authenticated sources in the fields of psychiatry and psychology as well as from the author's own clinical work.

The author is inclined in the interpretation of some of the cases presented to follow the Freudian school and introduces sex cases for examples when other types of cases might answer equally well. This, in the reviewer's opinion, would mitigate against the use of the text in certain colleges and universities and be apt to focus, needlessly, the attention of the students on that topic.

The reviewer does not imply that the topic does not need to be presented but that perhaps the present tendency is to overemphasize it.

As is the case with most of the abnormal psychologies written, too much space is devoted to purely psychiatric material, six chapters being given to this field. The chapters discussing the theories of sleep and dreams are good. Some of the late experimental results on sleep have been omitted and some aspects of dreaming other than the causal factors could well be included. In dealing with hypnotism the author has answered in a very satisfactory manner the questions usually raised by the student.

The topic of feeble-mindedness has been treated from a sociological rather than a psychological point of view, although the material presented will be valuable in preventing the confusion of amentia with certain types of mental deterioration. The glossary is complete and will serve as a ready reference for the student who likes short definitions for terms. There are a few definitions which are misleading and a few others which are tautological.

Johns Hopkins University ROY M. DORCUS

Insomnia: How to Combat It. By JOSEPH COLLINS. New York, D. Appleton & Co., 1930. Pp. vii, 131.

This book is one of the Appleton Popular Health Series, and is written in the well-known popular style of the author. Aside from what is given in a chapter on "Opiates and Narcotics" in a few sentences there is nothing in the book not well known to psychologists with a little self-doctoring experience. The aim of the book is, of course, to reach the layman. The author puts great emphasis on the facts that continued insomnia is only a symptom of something that must be corrected and that the use of narcotics (except under the careful direction of a physician) is a most dangerous practice: "No word of warning can be too loud and no protest too emphatic against the use of narcotics. They are all injurious and health destroying."

Our interest is not so much in the chapters on drugs and various physical electric treatments as in the advice that a physician gives on the psychological aspects of the subject in a book like this; for insomnia is largely a condition resulting from poor ordering of one's daily life. The author is careful to emphasize the fact that temporary insomnia is nothing to become excited about, thus aggravating the matter if it is due only to some mental shock or worry; that suggestion by another makes one too dependent, etc.: but like all books by physicians who have not been trained in sound biological psychology, this one shows how much is lost in such neglect of psychology in the physician's training. Even the physician's work is very largely a matter of effecting sound habits and consistent behavior. One is disappointed to see how much the author, even in a popular book, bases his arguments on the practices and sayings of poets and wise men of other centuries, even to the neglect of exceptionally valuable facts and material which might be obtained from modern scientific studies, such as Cannon's on emotional disturbances, or Jacobson's on 'progressive relaxation,' each bearing in many respects directly on the subject. The book, of course, does provide the layman with considerable sound advice.

J. P.

Introduction to the Study and Interpretation of Drama. By J. W. KAISER. Amsterdam, N. V. Swets & Zeitlinger, 1929. Pp. 77.

Drama, according to Kaiser, is born of a feeling of superiority and the desire to prevail. It is a justification—an unconscious one—of the dramatist's behavior pattern, a portrait of himself and his compensating struggle. Starting from an initial mood of realized inferiority, it moves along a rising line of self-confidence, recognition, dominance. The first act or movement is an exposition of the reason for the author's feeling of inferiority and ends in some decision designed to overcome this inferiority. The second act develops this decision with a duality in the rising action which leads to the crisis usually found in the third act. The fourth act develops the preference embodied in the crisis and the fifth act or conclusion stands for a reconciliation between the world and the hero; that is, the hero gains recognition.

Kaiser's exposition of this thesis is concerned mainly with the two plays *Cyrano de Bergerac* and *The Merchant of Venice.* The broad inadequacy from which Cyrano suffers is the lack of physical beauty which makes him despair of love and drives him to a restless struggle for superiority in every direction. To win the love of woman becomes indispensable to attaining dominance. Indirectly he gains his goal by a masked wooing of Roxane for his friend Christian. It would seem that Christian's success means Cyrano's failure, but the former's success is an ambiguous one which really demonstrates Cyrano's superior value. The action after the crisis is determined by need of revelation of the true situation and the recognition of Cyrano's dominance, a recognition which comes when Roxane declares her love for the beautiful soul (Cyrano) and when, after Christian's death in battle, Cyrano confesses openly his love of Roxane. The play as a whole is a portrait of a poet's attitude toward life, the glorification of denial of the reality of life for the sake of an apparent victory.

The interpretation of *The Merchant of Venice* as a development of the inferiority motive demands much more subtlety. Antonio is the tender-hearted friend brought into danger of death by the consequences of his generosity. Bassanio personifies the attributes precluded in Antonio's scheme of life, but which are nevertheless indispensable to the realization of indisputable superiority. The play reveals the veiled sacrifice of Bassanio for the sake of Antonio who would be king at any price; whose friends are mere shadows of himself, sharing his lack of appreciation of woman—an interpretation fitted also to Shakespeare himself.

The casket episode of *The Merchant of Venice* offers particular advantages for the development of unconscious motives. Morocco, Arragon and Bassanio are three possibilities revolved in one consciousness. Morocco with his "golden mind" chooses the golden casket, the choice nearest Antonio's own implicit desire, but less desirable for Bassanio who wins Portia by choosing the leaden casket. The ring episode indicates that although Bassanio in finding the ideal women has apparently surrendered the ideal represented by Antonio, the dominance is only apparent. Bassanio will surrender the ring given him by his wife with most significant words; the friend prevails.

The usual criticism of the manifold discovery of Freudian complexes in life and literature, that one can so select facts from a complicated plot as to prove

almost any preconceived theory, holds, too, for Kaiser's theory of the drama. His interpretations seem plausible enough, but no doubt might be countered by others equally plausible. This quality, perhaps, great drama shares with life.

Many of the episodes of *The Merchant of Venice* are to be found in the sources of the play, in the three old stories out of which the plot was woven. Both the casket and the ring device are taken from old material. The leaden casket is the chosen one in the legend, just as it is in Shakespeare's drama. Of course, Shakespeare's selection of incidents might be as significant as his creation of them. But has Kaiser reckoned with the sources? Or with the wholesale borrowing of material for plots and the rejuvenation of popular plans necessitated by the demands upon Shakespeare for rapid production? Possibly such pressure gave Shakespeare's Unconscious just the opportunity it craved. But how prove it?

Conflicts there must be in drama. Whether such conflicts be understood as unconsciously motivated personality-splits personified by the various characters of the drama, or real flesh and blood opponents, is perhaps immaterial. Dramatic character creation is a mystery which if understood might illuminate character creation as a social product. Any attempt to penetrate the motivating processes in such activity is probably worth while, but how, one wonders, is objective verification of the conjectured processes to be obtained?

University of Wyoming JUNE E. DOWNEY

Warum fragen die Menschen warum? Erkenntnistheoretishe Beiträge zur Lösung des Kausalitätsproblems. By WILHELM HEUER. 2d ed. Heidelberg, Carl Winter's Universitätsbuchhandlung, 1929. Pp. xi, 321.

Causal questions arise in the first place when an object, or a group of objects, fails to exhibit expected characteristics, *e.g.* when a doorbell which ordinarily rings fails to do so. From these questions it is only a step to those concerned with why an object exhibits expected characteristics, *e.g.* why does a doorbell ordinarily ring?

These answers to the titular question raise the question as to why we expect, and the attempt to answer this carries the author through the whole gamut of epistemological problems. In the course of pushing his problem farther and farther back, the author finally comes to a compulsive character of thought (Denkzwang) which leaves him practically at an impasse. This, in the reviewer's opinion, makes him say in substance that we ask why because we are compelled to, and, aside from the fascinating manner in which he has led from one question to another, he leaves the problem no nearer solution than at the outset.

Stephens College CARL N. REXROAD

Gedanken zur Naturphilosophie. By PAUL SCHILDER. Wien, Julius Springer, 1928. Pp. vi, 127.

The point of view of this book, as the author tells us in the preface, is philosophical. It is a philosophical appraisal of recent developments in natural science—physical, biological, and psychological.

The volume reveals a masterly knowledge of the major problems in advanced scientific thought today. The quantum theory, Einstein's relativity, the inheritance of acquired characteristics, and Lamarckian evolution are typical of the problems discussed. Statements of theory are well substantiated by facts, and there are frequent references to scientific literature.

The author's general philosophical position is frankly dualistic. The physical world is subsumed under the category of causality; the mental world, under that of purpose. The essence of the mental is its purposeful *Auf-einander-Bezogensein.* (Here we see the influence of Brentano's school.) This purposeful direction of the organism cannot be derived from physical formulae; it is something of an entirely different sort. Such a conception of nature is not to be confused with vitalism. Driesch's vitalistic theory is expressly rejected.

Although this book will naturally be of interest primarily to the philosopher, the psychologist with a speculative turn of mind will find in it much food for hard thought.

Bryn Mawr College E. FRANCES WELLS

1. *Child Study Discussion Records: Development—Method—Technique.* By MARGARET J. QUILLIARD. New York, Child Study Association of America, 1928. Pp. 74.

2. *A Selected List of Books for Parents and Teachers.* Selected and compiled by the Parents' Bibliography Committee. New York, Child Study Association of America, 1928. Pp. 78.

3. *Parents' Questions: My Child Will . . . My Child Won't . . . What Shall I do?* By CECILE PILPEL. New York, Child Study Association of America, 1928. Pp. 63.

These three pamphlets are published by the Child Study Association of America for use in parent-education. The first deals with the organization of study-groups for parents and with methods of conducting them. Purposes of the study-group, qualifications of the leader and suggestions for publicity campaigns are taken up in some detail. There are also a number of detailed lesson-outlines and verbatim records of group-discussion. While the material is extremely elementary, it should have some value for beginning workers in the field. The classified reading list comprises a good selection from the non-technical field. The third pamphlet is modeled after the well-known series of leaflets by Dr. Thom. A number of minor problems of child behavior are cited and possible causative factors discussed.

University of Minnesota FLORENCE L. GOODENOUGH

Five Types of Ethical Theory. By C. D. BROAD. New York, Harcourt, Brace & Co., 1930. Pp. xxv, 288.

The author's purpose is to "expound and criticise five types of ethical theory, viz., those of Spinoza, Butler, Hume, Kant, and Sidgwick. These systems are so different as to give together a fair idea of the range of possible views on the subject," and their authors are considered to be "of the highest rank." These moralists are presented, after brief biographical sketches of each, in successive chapters, followed by a chapter on conclusions. The essay

on Butler was formerly published in *Hibbert Journal*, and those on Spinoza, Hume and Kant comprised the *Donnellan Lectures* delivered in Trinity College, Dublin, in 1929. In the last chapter the author gives a sketch of the main problems of ethics and of the various kinds of theory considered possible about each of them. He divides all ethical theories into *naturalistic* (those which reduce ethical characteristics without remainder into non-ethical ones) and *non-naturalistic*. He declares his own adherence to the latter, regarding it as probable that ethical naturalism is false "and that ethical characteristics are *sui generis*." Broad regards it as "almost certain" that the concepts of 'right,' 'ought,' 'good,' etc. are *a priori* (p. 281). To the American psychologist the treatment will appear too theoretical and classificatory. Even the author himself admits that such theory has little, if any, relation to ethical conduct. The danger of over-simplification and finality in the treatment of this complex subject is rightly stressed. J. P.

University of Iowa Studies in Psychology, No. XII. Seashore Commemorative Number. Edited by WALTER R. MILES and DANIEL STARCH. Psychol. Monog. 39, No. 2. Princeton, Psychological Review Co., 1928. Pp. 223.

This monograph includes a note of appreciation of Professor Seashore's services at the University of Iowa, an annotated bibliography of all of his published writings, a review of all of the studies which have appeared in the first ten volumes of the Iowa Studies in Psychology, and seventeen new articles by Seashore's former students. The volume is an appropriate tribute to Seashore's career at the University of Iowa and to his work in the field of psychology generally.

Psychologists will appreciate having these summaries of the studies which have been issued from Seashore's laboratory. Their presentation in this concise form may aid in giving one a perspective of the scientific importance of the work on musical talent which Seashore has promoted. The new studies are divided into three groups, as follows: (1) studies in human behavior, (2) psychology and college instruction, (3) psychology and the artistic. The majority of them appear to have been published in substance in other places.

Vanderbilt University LYLE H. LANIER

A Brief Survey of Special Education in the Public Schools of Baltimore. By J. E. W. WALLIN. Preface by David E. Weglin. Baltimore, Department of Education, 1929. Pp. 57.

This report is the result of a brief survey (evidently about two weeks were spent in visiting schools, etc.) of the present organization of special classes in Baltimore. The results of the investigation are stated in a popular non-technical manner and major recommendations for improvement are made. Baltimore was found to have more retarded children and poorer facilities for their care than are found in certain Ohio cities. But there was also a larger percent of negroes.

Some of the twelve recommendations are: establishment of definite standards of admission to special classes; a dual system of special classes; arrangement for part-time school and part-time employment for older pupils, and an organization of the special classes and school into an independent Division of Special Education with a competent director in charge.

University of North Dakota C. W. TELFORD